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A BOOK OF MARVELS

BY
LIEUTENANT-COMMANDER
RUPERT T. GOULD, R.N.
(RETIRED)

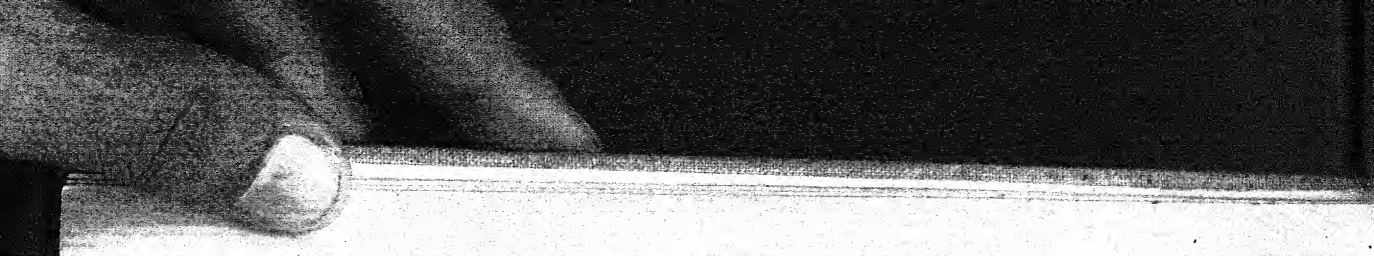
With illustrations by the Author

METHUEN & CO. LTD., LONDON

First published in 1937

PRINTED IN GREAT BRITAIN

TO
MISS J. BOWER



PREFACE

THESE seven essays have been selected from the twenty contained in my *Oddities* (1928) and *Enigmas* (1929), both of which books are now out of print.

Except for a few trifling corrections, I have left them unaltered; but in one or two cases I have added a post-script, containing information which has come to my notice since the essay was first published.

R. T. GOULD

ASHTEAD, 1937

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I

THE DEVIL'S HOOF-MARKS

A SCOTTISH minister once preached a sermon upon the text, "The voice of the turtle is heard in our land."[1] He was literally-minded, and unaware of the fact that the "turtle" referred to is the turtle-dove, and not that member of the *Chelonia* which inhabits the ocean and furnishes the raw material of such "tortoise-shell" articles as are not made of celluloid. In consequence, the deductions which he drew from his text were long remembered by such of his hearers as were better informed.

"We have here," he is reported to have said—"we have here, my brethren, two very remarkable signs and portents distinctly vouchsafed to us. The first shall be, that a creature which (like Leviathan himself) was created to dwell and abide in the sea shall make its way to the land, and be seen in the markets and dwelling-places of men; and the second shall be, that a creature hitherto denied the gift of speech shall lift up its voice in the praise of its Maker."

A visitation of a somewhat similar and hardly less startling kind occurred in Devonshire on February 8, 1855. The following account of it was published in *The Times* of February 16th.

"EXTRAORDINARY OCCURRENCE

"Considerable sensation has been evoked in the towns of Topsham, Lympstone, Exmouth, Teignmouth, and Dawlish, in the south of Devon,[2] in consequence of the discovery of a vast number of foot-tracks of a most strange and mysterious description. The superstitious go so far as to believe that they are the marks of Satan

[1] Canticles ii. 12.

[2] See Fig. 1.

himself; and that great excitement has been produced among all classes may be judged from the fact that the subject has been descanted on from the pulpit.

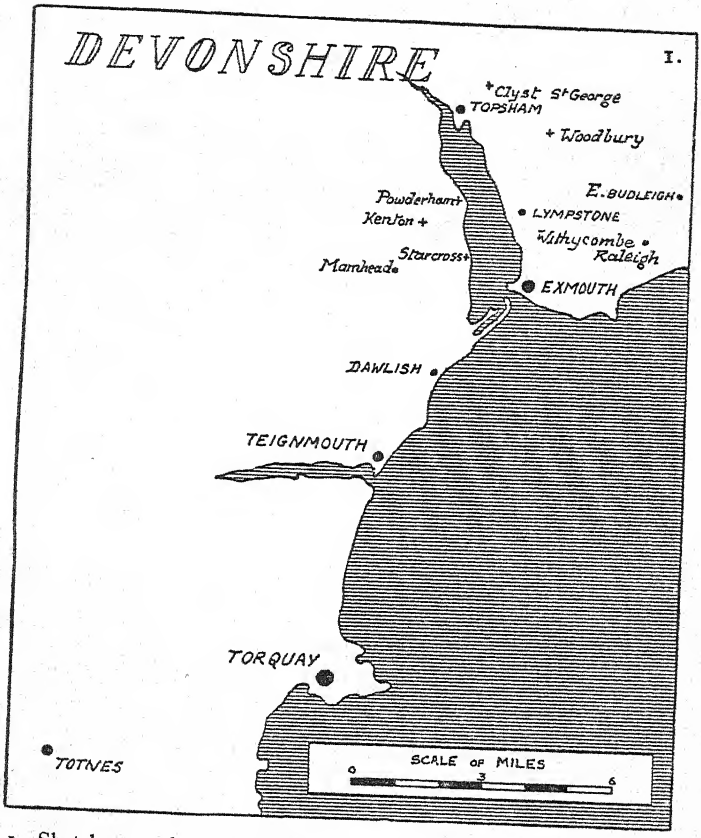
"It appears that on Thursday night last [Feb. 8, 1855, R.T.G.] there was a very heavy fall of snow in the neighbourhood of Exeter and the south of Devon. On the following morning, the inhabitants of the above towns were surprised at discovering the tracks of some strange and mysterious animal, endowed with the power of ubiquity, as the foot-prints were to be seen in all kinds of inaccessible places—on the tops of houses and narrow walls, in gardens and courtyards enclosed by high walls and palings, as well as in open fields. There was hardly a garden in Lympstone where the foot-prints were not observed.

"The track appeared more like that of a biped than a quadruped, and the steps were generally eight inches in advance of each other. The impressions of the feet closely resembled that of a donkey's shoe, and measured from an inch and a half to (in some instances) two and a half inches across. Here and there it appeared as if cloven, but in the generality of the steps the shoe was continuous, and, from the snow in the centre remaining entire, merely showing the outer crest of the foot, it must have been convex.[1]

"The creature seems to have approached the doors of several houses and then to have retreated, but no one has been able to discover the standing or resting point of this mysterious visitor. On Sunday last the Rev. Mr. Musgrave alluded to the subject in his sermon, and suggested the possibility of the foot-prints being those of a kangaroo; but this could scarcely have been the case, as they were found on both sides of the estuary of the Exe.

[1] Read "concave". On the facts stated, the centre of the foot making the impression must have been farther from the ground than the outer parts of the foot.

"At present it remains a mystery, and many superstitious people in the above towns are actually afraid to go outside their doors after night."



1. Sketch-map showing the localities in which the "Devil's Hoof-marks" were observed, February 8, 1855.

So far—and, unfortunately, no farther—*The Times*. The *Illustrated London News*, however, took up the question, and opened its columns to what proved to be quite

an extensive correspondence, which I have used as the source of most of the information here given. In the West Country the affair gradually blew over—although I believe that it is still well remembered. There was no repetition of the occurrence, but it took a long time for the "excitement" and "superstitious folly" to die down. One correspondent[1] speaks of

> " . . . labourers, their wives and children, and old crones, and trembling old men, dreading to stir out after sunset, or to go half a mile into lanes or byways on a call or message, under the conviction that this was the Devil's walk, and no other, and that it was wicked to trifle with such a manifest proof of the Great Enemy's immediate presence. . . ."

The correspondent presents, as might be expected, a curious medley of additional facts and half-baked theories. I will first summarize the facts, premising that *The Times* account, while giving a good outline of the events, necessarily omitted one or two very curious details.

An eye-witness, signing himself "South Devon",[2] sent in an able account, from which the following extract is taken.

> " . . . The marks which appeared on the snow (which lay very thinly on the ground at the time) and which were seen on the Friday morning, to all appearance were the perfect impression of a donkey's hoof—the length 4 inches by $2\frac{3}{4}$ inches; but, instead of progressing as that animal would have done (or as any other animal would have done), feet right and left, it appeared that foot followed foot, in a *single line*; the distance from each tread being 8 inches, or rather more—the foot-marks in every parish being exactly the same size and the steps the same length.

[1] He signed himself "G.M.M." (*Illustrated London News*, 3.3.1855).
[2] *Illustrated London News*, 24.2.1855.

"This mysterious visitor generally only passed *once* down or across each garden or courtyard, and did so in nearly all the houses in many parts of the several towns above mentioned, as also in the farms scattered about; this regular track passing in some instances over the roofs of houses, and hayricks, and very high walls (one 14 feet), without displacing the snow on either side or altering the distance between the feet, and passing on as if the wall had not been any impediment. The gardens with high fences or walls, and gates locked, were equally visited as those open and unprotected.

"Now when we consider the distance that must have been gone over to have left these marks—I may say in almost every garden, on door-steps, through the extensive woods of Luscombe, upon commons, in enclosures and farms—the actual progress must have exceeded a hundred miles. It is very easy for people to laugh at these appearances and account for them in an idle way. At present no satisfactory solution has been given. No known animal could have traversed this extent of country in one night, besides having to cross an estuary of sea two miles broad. Neither does any known animal walk in a *line* of single foot-steps, not even man.

"Birds could not have left these marks, as no bird's foot leaves the impression of a hoof, nor, even were there a bird capable of doing so, could it proceed in the direct manner above stated—nor would birds, even had they donkey's feet, confine themselves to one direct line, but hop here and there; but the nature of the mark at once sets aside its being the track of a bird.

"The effect of the atmosphere upon these marks is given by many as a solution; but how could it be possible for the atmosphere to affect one impression and not another? On the morning that the above were observed the snow bore the fresh marks of cats, dogs, rabbits, birds, and men clearly defined. Why, then, should a continuous track, far more clearly defined—so clearly,

even, that the raising in the centre of the frog of each foot could be plainly seen—why then should this particular mark be the only one which was affected by the atmosphere, and all the others left as they were?

"Besides, the most singular circumstance connected with it was that this particular mark removed the snow, wherever it appeared, clear, as if cut with a diamond, or branded with a hot iron; of course, I am not alluding to its appearance after having been trampled on, or meddled with by the curious in and about the thoroughfares of the towns. In one instance this track entered a covered shed, and passed through it out of a broken part of the wall at the other end, where the atmosphere could not affect it.

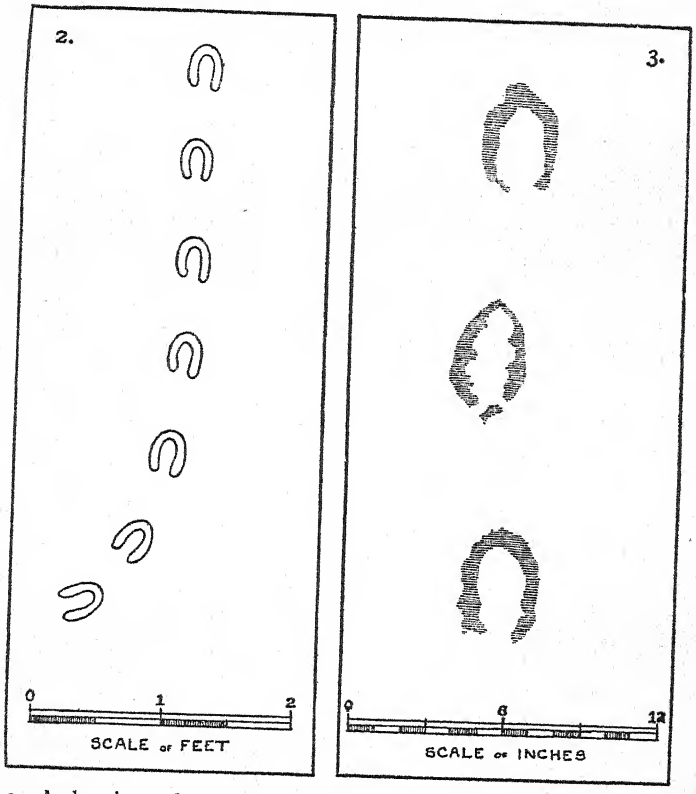
"The writer of the above has passed a five months' winter in the backwoods of Canada, and has had much experience in tracking wild animals and birds upon the snow, and can safely say he has never seen a more clearly-defined track,[1] or one that appeared to be less affected by the atmosphere. . . ."

Another correspondent, signing himself "G.M.M.", also afforded a good deal of supplementary information, as the following extracts will show.

" . . . As an amateur accustomed to make most accurate drawings from nature, I set to work soon after these marks appeared, and completed the accompanying exact facsimile[2] of those that were visible on the lawn of our clergyman's garden in this parish. He and I traced them through a low privet hedge, by a circular opening of 1 foot diameter. On applying a rule, the interval between each impression was found to be undeviatingly 8½ inches. This, in my opinion, is one of the most

[1] See Fig. 2, which is taken from a drawing accompanying his letter.
[2] See Fig. 3.

remarkable and confounding circumstances we have to deal with. . . .



2. A drawing of the hoof-marks, accompanying a letter signed "SOUTH DEVON", published in the *Illustrated London News* of February 24, 1855.
3. Another drawing of the hoof-marks, accompanying a letter signed "G.M.M.", published in the *Illustrated London News* of March 3, 1855.

"It was quite inexplicable that the animal, considering the scale of the foot, should leave, in single file, one print only, and as has already been observed, with intervals

as exactly preserved as if the prints had been made by a drill or any other mechanical frame.

"A scientific acquaintance informed me of his having traced the same prints across a field up to a hay-stack. The surface of the stack was wholly free from marks of any kind, but on the opposite side of the stack, in a direction exactly corresponding with the tracks thus traced, the prints began again! The same fact has been ascertained in respect of a wall intervening. . . . Two other gentlemen, resident in the same parish, pursued a line of prints during three hours and a half, marking their progress under gooseberry-bushes and espalier fruit-trees; and then missing them, regained sight of the impression on the roofs of some houses to which their march of investigation brought them. . . .

"I have addressed communications to the British Museum, to the Zoological Society, to the keepers of birds and beasts in the Regent's Park menagerie, and the universal reply is, they are utterly unable to form any conjecture on the subject, however correctly the impressions had been copied.

"I am emboldened to address you with more than the ordinary confidence of a correspondent 'well up in his facts', inasmuch as I am living in the centre of the district where the alarm, so to speak, was first given. Sir L. Newman's park, at Mamhead, is exactly opposite my own residence. Starcross Tower is an object of the picturesque, and beautiful to gaze upon from my study window: and Powderham Castle gleams in the sunshine half a mile further up. These are on the other side (west) of the River Exe, two miles in its breadth; and the marks were as abundant throughout the places just specified, and their neighbourhood—Kenton, Dawlish, Newton, etc.—as here at Exmouth, Withecombe Ralegh, Lympstone, Woodbury, Topsham, and the vicinity of Bicton and Budleigh. . . ."

In view of the very remarkable facts detailed in these letters, it will be admitted that the Devonshire rustics had every excuse for indulging in what their betters were pleased to term "superstitious folly". A natural explanation of the facts seemed impossible to find, and difficult even to suggest; while any explanation certainly postulated the visit of something very uncanny—something which walked upon small hooved feet with a very short, mincing stride, which sought darkness and solitude, which had never rested, which had covered something like a hundred miles in a single night, which had crossed a river two miles wide, which had hung round human habitations without daring to enter them, and which had on some occasions walked up walls and along roofs, while at other times it had passed through such obstructions as if they did not exist. Assuredly the peasants were not to be blamed if their minds went back to such grim texts as Isaiah xxxiv. 14:

> "The wild beasts of the desert shall also meet with the wild beasts of the island, and the satyr shall cry to his fellow."

Of course, many naturalistic explanations were offered, but none can be regarded as satisfactory. In the words of Maginn's *Aunciente Waggonere*,

> Somme swore itte was ane foreigne birde,
> Some sayd itte was ane brute. . . .

The various candidates who, by their "next friend", claimed the authorship of the marks comprised (among birds) cranes, swans, bustards, and waders; and (among beasts) otters, rats, hares, polecats, frogs, badgers, and—*mirabile dictu*—kangaroos.

Some of the theories were ingenious. For example, one Thomas Fox sent in a very clever drawing (Fig. 4) to support his view that the marks had been made by the four

feet of a leaping rat. There was a good deal, too, to be said for the otter theory. But the opinion most generally accepted was, of course, that put forward by the famous naturalist Richard Owen.

Here is his letter.[1]

"To the Editor of the *Illustrated London News*.

"An esteemed zoological friend has submitted to me a carefully-executed drawing of one of the more perfect impressions left in the snow at Luscombe, South Devon, on or about the 8th of last month. It was of the hind-foot of a badger. This is almost the only plantigrade quadruped we have in this island, and leaves a foot-print larger than would be supposed from its size.

"The sketch, of which you have given a cut in p. 187[2] (Feb. 24th), gives a correct general idea of the shape and proportion of these foot-prints, but without the indications of the pads on the sole, and the five small claws, which the drawing sent to me exhibited. Such perfect foot-prints were rare, because those of the fore- and hind-foot are commonly more or less blended together, producing the appearance of a line of single footsteps; which appearance, if a bear had been abroad in the five winter months spent by your correspondent in Canada, would have shown him was not peculiar to the foot-steps of man, but characteristic of other plantigrade mammals, though they may be quadrupedal. The badger sleeps a good deal in his winter retreat, but does not hibernate so regularly and completely as the bear does in the severer climate of Canada. The badger is nocturnal, and comes abroad occasionally in the late winter, when hard-pressed by cold and hunger; it is a stealthy prowler, and most active and enduring in its quest of food.

"That one and the same animal should have gone over a hundred miles of a most devious and irregular route

[1] *Illustrated London News*, 3.3.1855. [2] Fig. 2.

in one night is as improbable as that one badger only should have been awake and hungry out of the number concealed in the hundred miles of rocky and bosky Devonshire which has been startled by the impressions revealed by the rarely spread carpet of snow in that beautiful county.

"The onus of the proof that one creature made them in one night rests with the assertor, who ought to have gone over the same ground, with a power of acute and unbiased observation, which seems not to have been exercised by him who failed to distinguish the truly single from the blended foot-prints in question.

"Nothing seems more difficult than to see a thing as it really is, unless it be the right interpretation of observed phenomena.

"RICHARD OWEN"

In the mid-Victorian era, that "period of digestion", the authority of an established name counted, in scientific as in other matters, for more than it does now. Probably all but a very few, such as the unfortunate observers who saw something different from what Owen so clearly tells them they ought to have seen, regarded this letter as absolutely decisive.

Nowadays, we know a little more about scientific dogmatism—and we also know a good deal more about Owen himself. He was, undoubtedly, a very great man; but on several important occasions he showed himself capable of making dogmatic assertions, in defiance of fact, which proved him to be possessed of a singular and not entirely "scientific" type of mind.

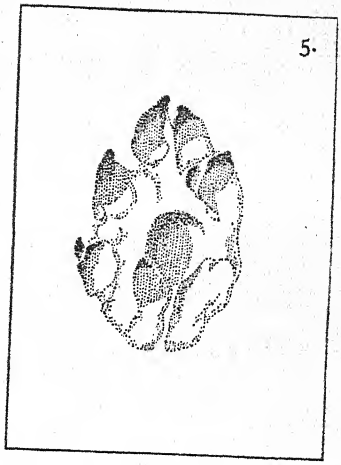
A good example of this tendency is his controversy with Huxley, in 1857, over the *hippocampus major*. Owen, coming foward "on the side of the angels" as the great scientific gun of the anti-Darwinians, committed himself to the dogmatic assertion that there were certain anatomical

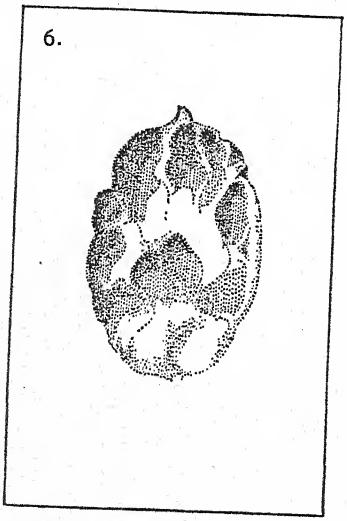
features—such as the above singularly-named structure—which were peculiar to the brain of man, and afforded ample ground for classifying him as a genus apart from all other mammals. Actually, as Huxley soon afterwards showed, such structures are common both to man and to all the higher apes, as well as many of the lower ones.

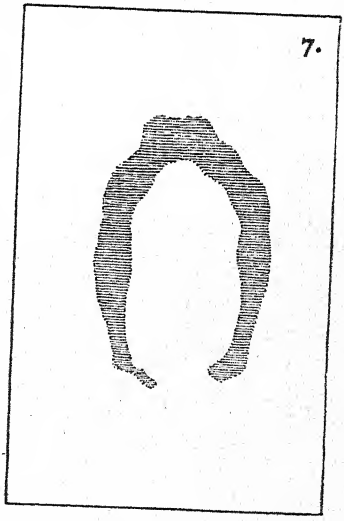
Proxime accesserunt may be placed Owen's exploded theory that the adult skull is a modified vertebral joint—a theory originally suggested by Oken—and his utterly childish "explanation" of the "sea-serpent" seen by H.M.S. *Dædalus* in 1848: an explanation flatly contradicting the observed facts, and postulating that the naval officers who observed them were, one and all, half-witted.

His explanation of the Devonshire hoof-prints is more plausible; but it does not fit the facts—nor is he fair to "your correspondent". "South Devon" nowhere stated, as Owen asserts, that man is the only creature which makes single foot-prints in snow—he said that no creature, not even man, makes a *single line* of foot-prints: and this is perfectly true. It is quite possible that the prints of a badger's hind-foot might be super-imposed on the last impression but one made by the fore-foot on the same side of the body, and so produce an apparently single foot-print. But such prints would undoubtedly be "staggered", for the badger has quite a wide "tread", and the result would then be a double line of imprints, not a single one. Badgers, also, are not commonly credited with the ability to scale walls and walk along roofs. As between the claims of the badger and the otter, the latter certainly seem better founded.

In Figures 5 and 6 I have drawn foot-prints of a badger and an otter for comparison with the Devonshire hoof-marks. It will be admitted that the resemblance is not striking. It is only fair, however, to say that one or two of the writers to the *Illustrated London News* stated that







4. From a letter, signed "THOMAS FOX", in the *Illustrated London News* of March 10, 1855, suggesting that the hoof-marks were made by a leaping rat.
5. Impression of an otter's hind-foot.
6. Impression of a badger's hind-foot.
7. One of the hoof-marks, enlarged from Fig. 3.

faint traces of claws had, as Owen remarks, been seen, or imagined, at the edges of the hoof-marks.[1]

But, putting aside the reported facts which are inexplicable on any naturalistic theory (such as the unobstructed passage of the tracks through walls, etc.), there is a crucial objection which appears to me to dispose of the claims not only of the badger and the otter, but of *all* the birds and animals supposed, by some one or other, to have made the mysterious marks. I except the kangaroo —that theory does not require serious discussion. It was only mooted, originally, because the private menagerie of a Mr. Fische, at Sidmouth, contained a couple of these animals.

The objection is this. Whatever made the hoof-marks, it is quite certain, from the alarm they occasioned among the rustics, that they were most unusual—that nothing like them had ever been seen within living memory. It is therefore indisputable that they were not made by any common beast or bird. If such had been the case—if, for example, they had been the foot-prints of a badger or an otter—they would have been seen in Devonshire every winter. Instead of being a nine days' wonder, and scaring the feebler brethren into fits, they would have been looked upon as a perfectly familiar sight, not worth a second glance. Yet, with one exception, there is no record of such marks having been seen on any other occasion before or since.

Unbeknown, apparently, to the correspondents in the *Illustrated London News*, a very similar case had occurred some fifteen years earlier, in a very different part of the world. The story had been published for eight years; but it is a curious fact that while the authority for it, Rear-Admiral Sir James Clark Ross, R.N., was in England in 1855 and must, one would think, have heard of the

[1] e.g., "Ornither" and an anonymous correspondent, both of whose letters appeared on March 3, 1855. The correspondence terminated on March 17th.

Devonshire hoof-marks, he did not, apparently, direct attention to the very similar incident which had come to his own knowledge.

The following is an extract from Ross's *Voyage of Discovery and Research in the Southern and Antarctic Regions*, vol. i, p. 87. His ships, the *Erebus* and *Terror*, were then at Kerguelen Island, a large sub-antarctic island in the Southern Indian Ocean. The date is May 1840.

> "Of land animals we saw none; and the only traces we could discover of there being any on this island were the singular foot-steps of a pony or ass, found by the party detached for surveying purposes, under the command of Lieutenant Bird, and described by Dr. Robertson as 'being 3 inches in length and 2½ in breadth, having a small and deeper depression on each side, and shaped like a horseshoe'.
>
> "It is by no means improbable that the animal has been cast on shore from some wrecked vessel. They traced its footsteps for some distance in the recently fallen snow, in hopes of getting sight of it, but lost the tracks on reaching a large space of rocky ground which was free from snow."

One wonders, if they had "got sight of it", what they would have seen.[1]

It is scarcely a far-fetched conjecture to suppose that the creature which made the "singular foot-steps" seen by Ross was akin to that whose tracks were observed in Devonshire. If we accept this, one or two conclusions seem to follow.

[1] Dr. R. M'Cormick, R.N., who was supposed to be the official zoologist (and geologist) of Ross's expedition, does not refer to these marks in the account of the voyage given in his *Voyages of Discovery in the Arctic and Antarctic Seas, and Round the World* (London, 1884). It is probable, however, that he never saw them himself (his journal at Kerguelen is mostly devoted to a trivial and querulous account of his teal-shooting expeditions): and he was not the man to give prominence to the work of others. His book, also, was published forty years after the voyage.

The Kerguelen creature was not a denizen of Kerguelen itself—at least, what we now know of the fauna of that island makes this exceedingly improbable. Presumably, then, it made its arrival from seaward. Either, as Ross suggests, it was a survivor from some wrecked vessel, or it was a sea-creature which, for some reason, had made an excursion on land.

The locale of the Devonshire hoof-marks points to a similar conclusion. All the places mentioned by name lie, as will be seen from Fig. 1, close to the sea-coast or to the estuary of the Exe. As to what manner of sea-creature it may have been, if it was one, I offer no opinion. The available selection is wider than might be at first supposed—it may be recalled that some years ago a seal was found half-way up a Scottish mountain, and miles from the sea.

On the other hand, it is possible that in both cases the agent was a land-animal—presumably a tropical land-animal.[1] The appearance of its foot-prints in snow would normally be a matter of inference, rather than observation, while they would never, except by a rare accident, be observed in either of the temperate zones. A land-animal swimming ashore from a ship would naturally seek for food—and, if timid, might easily cover a very considerable distance in a single night, and hang round buildings without daring to enter them.

On either supposition, it is possible that there is some quite simple solution of the Devonshire hoof-marks to be found, if one knew where to look for it. But there is a caveat to be entered. If a land-animal made the marks, the available data are probably sufficient to enable a competent zoologist, with an unbiased mind, to make a reasonable suggestion as to its identity. But no authority on earth—not even the Ministry of Agriculture and

[1] It was not, obviously, a common denizen either of the British Isles or of Kerguelen; localities whose climates are respectively temperate and sub-polar.

Fisheries—can set limits to the number and variety of the creatures which, even though unknown to science, may yet live and move and have their being in the sea.

Postscript

An anonymous pamphlet, *The Swan with the Silver Collar*, published at Wells in 1855, is of interest as showing the great interest aroused in the West Country at the time. I am inclined to identify its author with a correspondent of the *Illustrated London News*, one "W. W., Somerset", whose letter was referred to, but not printed, in the issue for March 3, 1855.

His facts, if correctly stated, are interesting, but his theory is of no value. Apparently, five days after the appearance of the Devonshire hoof-marks a swan turned up, alive but exhausted, at St. Denis in France, having round its neck a silver collar "with an inscription engraved on it, stating that the bird belonged to the domain of Prince Hohenlohe, in Germany". Our author concludes that this bird, whose feet had probably been "strongly and thickly padded" by its owner, to prevent damage to the garden where it was normally kept, must have made the mysterious marks! Oddly enough, a letter to Prince Hohenlohe, raising this point, met with no reply.

II

THE VAULT AT BARBADOS

In the churchyard of Christ Church,[1] Barbados, overlooking Oistin's Bay, stands a small but massive vault, differing in no very obvious way from its neighbours except that there is no stone or other barrier at its entrance. The passer-by, looking in, sees nothing but bare walls and casual rubbish. The vault has stood thus, open and untenanted, for over a century—and until, if ever, the traditions relating to it have perished there is little doubt that it will remain so.

For it is not, apparently, a quiet resting-place for the dead. Not once nor twice, but five times in all, at intervals varying from a few months to several years, have coffins entrusted to its keeping been found, when it was next opened, to have been overturned, scattered in confusion, and even set on end. All conceivable precautions have been taken—the walls, roof, and floor sounded for concealed passages, the floor sanded to detect foot-marks, the entrance closed with a block requiring four men to move it, and that block sealed and marked by several independent persons. The result has always been the same. At the next opening, while there has never been a trace of any human being having made the smallest attempt even to reach the interior, the coffins, on each occasion, have undergone the same shameful treatment as before. It is not wonderful that after the fifth occurrence of the kind (in 1820) the dead whom the vault could not protect should, by common consent, have been removed to a more peaceful asylum elsewhere.

[1] Christ Church was destroyed in 1831 by a hurricane. The foundation-stone of a new building was laid on October 1, 1835, and that of a third exactly a century later—the 1835 church having been burned down in 1935.

The story has often been told in print. Several versions exist, which differ slightly in detail, but are in remarkable agreement as to the main facts. Discrepancies and agreement alike, however, are capable of a simple explanation. Practically all the versions, excluding such as are obviously mere repetitions at fifth-hand or so, can be traced back to one account—or, rather, several varying transcriptions of one account—written by the Rev. Thomas H. Orderson, Rector of Christ Church during the whole period of the disturbances in the vault (1812–20). Mr. Orderson seems first to have drawn up a complete account for his own reference and then, at different times, to have made several copies of this for curious correspondents. As time went on, he may have grown weary of the whole subject; at any rate, the versions which he circulated do not agree in all respects—for example, one which has been declared[1] to be "absolutely authentic" (which it is, though no more so than several similar documents) omits an important interment in the vault altogether, and gives a wrong date for the first appearance of the disturbances.

While, however, Orderson's narrative provides the backbone of the evidence, we are not, as will be seen, entirely dependent on it. There is a considerable body of other testimony, which will be discussed in its place. In brief, it may be said that the evidence for the bulk of the happenings within and without the vault during the period of the disturbances is quite unassailable.

The first appearance of the story in print appears to have been in 1833, when Sir J. E. Alexander gave a short résumé of the facts in his *Transatlantic Sketches*. As his account, although short and incomplete, gives a useful introduction to the question, I reprint it here.

"It is not generally known that in Barbadoes there is a mysterious vault, in which no one now dares to deposit

[1] By the late F. M. Alleyne. See Sir A. Aspinall's *Pocket Guide to the West Indies* (London, 1923), p. 97.

the dead. It is in a churchyard near the seaside. In 1807 the first coffin that was deposited in it was that of a Mrs. Goddard; in 1808 a Miss A. M. Chase was placed in it; and in 1812 Miss D. Chase. In the end of 1812 the vault was opened for the body of the Honourable T. Chase; but the three first coffins were found in a confused state, having been apparently tossed from their places. Again was the vault opened to receive the body of an infant, and the four coffins, all of lead, were much disturbed. In 1816 a Mr. Brewster's body was placed in the vault, and again great disorder was apparent in the coffins. In 1819 a Mr. Clarke was placed in the vault, and, as before, the coffins were in confusion.

"Each time that the vault was opened the coffins were replaced in their proper situations, that is, three on the ground side by side, and the others laid on them. The vault was then regularly closed; the door (and a massive stone which required six or seven men to move) was cemented by masons; and though the floor was of sand, there were no marks of foot-steps or water.

"The last time the vault was opened was in 1819. Lord Combermere was then present, and the coffins were found confusedly thrown about the vault, some with the heads down and others up. What could have occasioned this phenomenon? In no other vault in the island has this ever occurred. Was it an earthquake which occasioned it, or the effects of an inundation in the vault?"

Schomburgk, in his *History of Barbadoes*,[1] published in 1844, gives a similar version, adding that at the time of the opening in 1820 (the correct date of Alexander's "last time the vault was opened") a sketch was made of the disarray of the coffins. He also states that the sand

[1] This is still the more usual spelling, but the accepted official form is "Barbados". The origin of the name is uncer[illegible]

on the floor was deliberately put down for the purpose of detecting foot-prints.

In 1860 a pamphlet with the alluring title of *Death's Deeds*, giving an account of the disturbances in the vault, was printed in England. It was anonymous, but appears to have been the work of a Mrs. D. H. Cussons. It formed the basis of another account printed in the *Memoirs and Correspondence of Field-Marshal Viscount Combermere*. I regard both accounts as entirely untrustworthy. The pamphlet I have not seen; nor do I know whether the inflated style of the Combermere account is native or transplanted. Judging by the clap-trap title of the Cussons pamphlet, there is no difficulty in believing it to be the fountain-head from which flowed the annexed "old abusing of God's patience and the King's English":

> "When they endeavoured to remove the stone it resisted with unwonted weight. . . . For a moment all hands were paralysed, and a look of wondering dismay passed from each to each; but it was only for a moment. The next, excitement lent a powerful energy to their efforts, and the stone yielded half an inch, enough to afford a glimpse inside. Nothing was distinctly visible in the darkness of its buried night. Still, the light which entered through the narrow crevice seemed to cut across some black object close to the portal, so near that the thread-like ray lay brilliantly visible, prevented by this massive black substance from dispersing itself into the reigning darkness within.
>
> "Terror a second time palsied the energies of those engaged in this operation. Suspense deepened the intensity of interest and awe which transfixed the anxious spectators. Every breath was hushed lest they should fail to catch the first whisper of those near the tomb that might afford a solution to the problem before them."

And much more, in the style of Mrs. Amanda McKitrick Ros. Actually, this turgid narrative is an attempt to relate that when the vault was opened for the last time, in Lord Combermere's presence, one of the coffins was found to be jamming the door; an event which, as shown later, probably occurred only in the imagination of the authoress.

In the early years of this century the late Andrew Lang made a careful examination of the available evidence, with the aid of several MS. accounts of the disturbances. He gave a very complete account of his results in the *Folk-Lore Journal* for December 1907, in an article entitled "Death's Deeds; a Bi-located Story", which I regard as the fairest and clearest account of the whole matter extant.

The story was also retold by Sir Algernon Aspinall, in an essay, entitled "A Barbados Mystery," forming part of his book *West Indian Tales of Old*. He also summarized it in his *Pocket Guide to the West Indies*. It is to be regretted that the particular Orderson version used by him was, as can easily be seen, erroneous in two important particulars.[1] Following him, Sir Arthur Conan Doyle gave a short and similarly inaccurate account in an article, "The Law of the Ghost", which appeared in the *Strand Magazine*, forming part of a series with the general title of "The Uncharted Coast".[2]

I turn now to the MS. sources of information, most of which, as previously remarked, are slightly different versions of the facts from the standpoint of a principal witness—all written by Orderson, and communicated by him at various times to different persons. In fairness, I should add that I have not had the opportunity of seeing any of these MSS.; I take the details of them from Lang and from Sir A. Aspinall.

[1] It states that the coffins were first found disturbed on July 6, 1812 (interment of Dorcas Chase): and it omits the interment of Thomas Chase on August 9, 1812. It was on the latter occasion that the disturbance of the coffins was first noted.

[2] No. 348, vol. 58, December 1919.

The burial register at Christ Church, which was examined by the late Hon. Forster M. Alleyne (Lang's brother-in-law), contains a complete record of the various interments in the vault, signed by Orderson, but no indication that anything out of the common had ever occurred there. The Parochial Treasurer's accounts, and the files of contemporary newspapers, were similarly found to be barren of interest.

He discovered, however, a description by an eye-witness, the Hon. Nathan Lucas, of the scene at the last opening of the vault in April 1820. This document, which also embodies one of the Orderson versions, is printed in full in Sir A. Aspinall's *Pocket Guide to the West Indies*. Mr. Alleyne also found allusions to the subject of the disturbances in the correspondence of a member of his family in 1820, and it is quite possible that more material of the kind may still be brought to light.

As to the Orderson versions, one (called "A" by Lang) was, in 1907, in the possession of the Hon. F. M. Alleyne already mentioned. His father, Mr. Charles Thomas Alleyne, had been in the island in April 1820, when the vault was opened for the last time. F. M. Alleyne told Lang that he had heard the story from the lips of an eye-witness of that event, Sir Robert Bowcher Clarke. "A" contained sketches of the coffins, both in order and when disarranged. On the back of one of the sketches was written "J. Anderson, Rector"—probably a mistake for "Orderson".

Another version (called by Lang "O") was printed for private circulation by one Robert Reece, who afterwards contributed an article on the subject to *Once a Week*.[1] This version formed the basis of the account given in

[1] March 11, 1865. This article gave rise to a controversy between Reece and one John Arnold, who maintained, ably but erroneously, that the disturbances at Barbados had been caused by surface-water flooding the vault. See *Once a Week* for 22.4.1865, 13.5.1865, 27.5.1865 and 12.8.1865.

A letter from Reece to a Major Clarke, describing the disturbances, also appeared in *The Lamp* for June 1864.

Death's Deeds, and, hence, also of that in the *Memoirs of Lord Combermere*.

A third version, which Lang does not name, but which it will be convenient to call "B", since it is "written on thin blue paper" (following the celebrated sergeant-major who filed the Wesleyan parade-states under letter H because they fell in at Half-past nine), was sent to Lang by F. M. Alleyne, who had copied it from a document once in the possession of a sister of Sir R. Bowcher Clarke. This version, signed by "Thomas H. Orderson, Rector", Lang prints in full, collating it with his "A" and "O".

The following chronological account, containing what I hope will be found a fairly faithful narrative of the events at the vault, has been compiled from all the foregoing materials. As is natural, most weight has been attached to the MS. documents—Lucas's statement and a synoptic combination of "A", "O", and "B". The printed versions have been used chiefly as comments upon these, but here and there a detail which they omit, and which is not in contradiction to them, has been adopted.

First, a few details of the vault itself. It was built early in the eighteenth century. It is partly above ground and partly below, the lower half being excavated, to a depth of about 2 feet, out of the solid limestone rock (which is common in most parts of Barbados). The upper portion is of exceedingly solid masonry, composed of large blocks of the local coral most firmly cemented together. The roof, seen from the inside, is arched; from the outside it appears flat. The sides have a slight inward slope as they rise. The floor-space is 12 feet long by $6\frac{1}{2}$ feet broad. The back is composed of two separate masonry walls, an inner and an outer, not united. The entrance is made through a doorway, formerly kept closed by a large slab of blue Devon marble resting against the sloping sides. From this entrance a few steps lead down into the vault.

On the tombstone is, or was, an inscription recording

that the vault was erected by the "truly sorrowful widow" (*née* Elizabeth Walrond) of the Hon. James Elliot, who was "snatched away from us the 14th of May, Anno Domini 1724", and who (as the student of epitaphs, particularly eighteenth-century ones, will readily anticipate) was "lamented by all who knew him".

Curiously enough, although the vault was undoubtedly erected for Elliot's benefit, he does not seem to have been interred there; or else his coffin was removed later. The first recorded interment in the vault is that of Mrs. Thomasina Goddard, on the 31st of July 1807 . . . "and when it was opened for her reception it was quite empty, without the smallest appearance of any person having been buried there". Mrs. Goddard's coffin appears, although the evidence is not conclusive, to have been of wood—a point which, as will be seen, is of some importance.

It may be noted that Lucas, in his statement, draws attention to a difference between the English and Barbadian practices of coffining the dead. In Barbados, apparently, it was then the practice to enclose the body first in a wooden coffin and then, at the grave, in an outer leaden one; not, as we do, to have first a wooden shell, then the leaden coffin, and then an outer one of wood. Mrs. Goddard's may have been a single one of wood, without the usual leaden covering. With one exception (noted), all the succeeding coffins placed in the vault had leaden outer shells.

How Mrs. Goddard came to be buried in the Elliot vault does not appear; but by 1808 the vault seems to have passed into the possession of the Chase family, three members of which were buried there between 1808 and 1812.

The first was Mary Ann Maria Chase, the infant daughter of the Hon. Thomas Chase, who was buried on February 22, 1808. Mrs. Goddard's coffin was found undisturbed.

The second was Dorcas Chase, also a daughter of

Thomas Chase, who was buried in the vault on July 6, 1812. She appears to have been an adult,[1] but there is no information as to her age. On this occasion, also, the coffins (two) already in the vault were found undisturbed.

The case was otherwise when the coffin of Thomas Chase himself was brought to the vault on August 9, 1812. The coffins of M. A. M. Chase and Dorcas Chase were found to have been displaced, that of M. A. M. Chase having been thrown from the north-east corner of the vault across to the opposite angle, where it had lodged almost on end, head downwards.

The negroes employed in the work of bestowing the coffins appear, with good reason, to have been much alarmed; but little notice seems, at the time, to have been taken of their stories. As it was obviously not their business to demand an inquiry into reports which, *prima facie*, convicted them or their predecessors either of negligence, theft, or practical joking, the matter seems to have blown over for the time; although this may have been the first occasion on which special precautions were taken to ensure that the vault was securely closed.

A second infant, Samuel Brewster Ames, was buried in the vault on September 25, 1816—an interval of just over four years since the last interment. All the coffins (except, possibly, that of Mrs. Goddard) were found in confusion.

It is a little difficult, even with the aid of the various narratives (or possibly, it may be thought, with their hindrance), to determine whether, in fact, the coffin of Mrs. Goddard was disturbed as well as the remainder. The point is of some little importance, since it has been suggested by Sir Arthur Conan Doyle that the forces inside the vault, whatever they were, betrayed antipathy to the leaden coffins only, and left Mrs. Goddard's, which was of wood, alone. I reserve the question for discussion later.

[1] Or, as a young French lady once phrased it in her anxiety to speak correct English, "an adulteress".

Thomas Chase's coffin, be it noted (about whose disturbance at this date there is no question), was an exceedingly heavy affair, requiring eight men to lift it. It had an outer leaden shell, and its occupant is stated to have been of great bulk and weight.

On November 17th of the same year (1816) the vault was reopened to receive the coffin of Samuel Brewster (a different person from S. B. Ames above), who had been murdered in an abortive slave-rising in the preceding April and temporarily buried at St. Philip. On this occasion the same confusion was observed among the coffins (which, of course, were carefully and reverently replaced every time they were found disarranged).

It is a little difficult to understand how the vault, which until 1816 had been, except for its first occupant, reserved for members of the Chase family, came now to be used, apparently, as the burying-place of several other persons of differing names. They may all have been relations; but on this point the available information gives no light.

On July 17, 1819 (so "A" and "O": "B" and Lucas say the 7th), Thomazina Clarke was buried in the vault in a wooden coffin (all the records are in agreement on this point; "B" states that it was of cedar). The coffins were again found in disorder.

This was the last burial in the vault. By this time public attention was fully roused. Lord Combermere, Governor of Barbados, was present at the interment, attended by his aides-de-camp and a considerable crowd. The interior of the vault was carefully examined and sounded, but no trace of any secret entrance could be found. The floor was next covered with sand, and after the coffins had been carefully replaced in rows the vault was closed, and the slab at the entrance cemented. Lord Combermere put his seal on it, and several witnesses added private marks of their own.

It may be noted that as the vault only measured 12 feet by 6½ feet in plan, six coffins (even though three were

those of children) must have needed careful stowage. The accounts and sketches of their arrangement vary slightly; but it seems generally agreed that the three largest coffins were laid side by side on the floor of the vault, and the other three arranged one on top of each of these. The only divergent account is that given by Lucas, who states: "The Children's coffins were placed on bricks in the Vault; Mr. Chase's on the Rock, the bottom of the Vault." This is entirely at variance with the sketches accompanying his account.

The seventh coffin, that of Mrs. Goddard, had by this time decayed and fallen to pieces. The "shakings" were tied together in a bundle, and left inside the vault, stacked against the wall. What had become of the body does not appear.

Some eight months elapsed, and then, on April 18, 1820, the vault was reopened in the presence of Lord Combermere and of "two other persons of the first respectability" ("B").[1] The determining cause of this opening of the vault is uncertain—I imagine it to have been simply curiosity, stimulated by the accident of Lord Combermere's presence in the neighbourhood. This, at any rate, is the reason given by Lucas. The "B" Orderson version states that Combermere determined to have the vault reopened upon hearing that a noise had been heard in it by night—interesting, if true, but not confirmed by any of the other versions. At all events, the opening was undoubtedly deliberate, and made for the express purpose of seeing whether the coffins had been again disturbed—not of adding to their number.

The coffins were found in a bewildering state of confusion—bewildering, because the seal and private marks on the slab and other parts of the vault were found perfect and undisturbed, while no trace of foot-marks appeared

[1] Lucas's statement is more specific:

". . . The Rector, the Rev. Dr. Thos. H. Orderson, . . . very soon arrived. His Lordship, myself, Robert Bowcher Clarke, and Rowland Cotton, Esq., were present during the whole time."

on the sanded floor. The remains of Mrs. Goddard's coffin stood, as they had been left, against the wall.

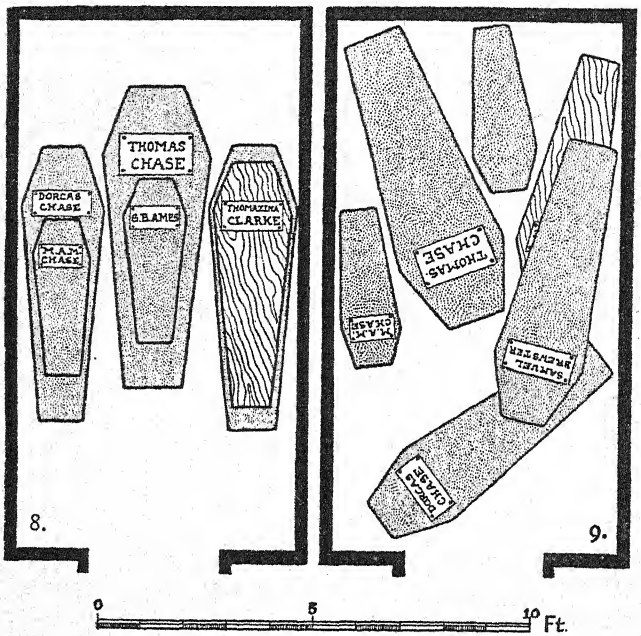
It was enough. Combermere—who had fought all through the Peninsular War with Wellington and, but for a piece of official jobbery, would have commanded the cavalry at Waterloo—was not, it will be conceded, a man easily daunted; but he seems to have concluded that the matter was beyond his powers as Governor to unravel, and to have offered no opposition when the Chase family gave instructions for the vault to be cleared and left empty, the coffins being buried elsewhere. One imagines that if Combermere had had any suspicion that trickery could have been practised, he would have made it his business, in view of the great popular excitement caused by the proceedings at the vault, to have the problem investigated further.

Such, in outline, were the happenings at the Chase vault, Barbados, during the years 1808–20.

As previously mentioned, sketches were made, when the vault was opened for the last time, showing the state in which the coffins were found, and that in which they had previously been left. Unfortunately, there are at least two separate sets of these sketches extant; and they conflict in a very puzzling manner.

Drawings based on one set of these sketches are shown in Figs. 8 and 9. The sketches annexed to the Orderson version forming part of Lucas's statement have been published by Sir A. Aspinall in the two works already mentioned. The originals are in very exaggerated perspective, and appear to be drawn as if looking into the vault from the entrance. The second of these sketches, which shows one of the coffins (that of Thomas Chase) leaning against the back wall, is probably the foundation of the story, apparently related in *Death's Deeds* and repeated in the Combermere memoirs, that this coffin, when the vault was last opened, was found jamming the door. Neither Lucas nor Orderson, who were both present,

makes any mention of this occurrence, which it is quite impossible that they could either have forgotten or failed to notice. I regard the statement as a pure fiction—a



8. Sketch showing the positions in which the coffins were left in the Chase vault, Barbados, on July 17, 1819—the vault being closed and sealed immediately afterwards.

9. Sketch showing the positions in which the coffins were found when the vault was reopened on April, 18, 1820.

misguided attempt to add to the gruesome horrors of a plain tale which needs no such embellishment.

Figures 8 and 9 are based upon sketches published by Lang in *The Folk-Lore Journal*, and taken from "B".[1]

[1] A third pair of sketches, agreeing practically *in toto* with these, were published by Reece in his *Once a Week* article. He states that

Personally, I regard them as more authentic than Lucas's, because being in plan, and in simple outline without detail, they look much more like sketches made on the spot and at the time, which both sets of sketches profess to be. The Lucas sketches, with their complications of perspective and detail, have an air of afterthought. That, however, is a matter of personal preference. It does not seem possible to reconcile the discrepancies between the two sets of sketches.

For the sake of completeness, it may be as well to give now some account of various similar events recorded as having happened elsewhere—for the Barbados mystery, although the most completely documented and the best-authenticated case on record, is not the only one. There are at least three others—although one is more than a little suspect.

Of these three, two occurred in England. Of the first, which seems to have happened about 1810, nothing more is known than can be gathered from the following extract from the *European Magazine* for September 1815:

"The Curious Vault at Staunton, Suffolk

"On opening it some years since, several leaden coffins with wooden cases, that had been fixed on biers, were found displaced, to the great astonishment of many of the inhabitants of the village. The coffins were placed as before, and properly closed; when, some time ago, another of the family dying, they were a second time found displaced; and two years after they were not only found all off the biers, but one coffin, as heavy as to require eight men to raise it, was found on the fourth step that leads into the vault.—Whence arose this operation, in

his father ". . . some years since, had two models constructed, exhibiting the various displacements to which the coffins were subjected; one of these models he presented to the late Bishop Blomfield, who took a great interest in this mysterious matter of the vault; the other is, I believe, deposited in the British Museum."

which, it is certain, no one had a hand? N.B. It was occasioned by water, as is imagined, though no signs of it appeared at the different periods of time that the vault was opened."

So far as I know, the text of this passage (here given verbatim), has not previously been printed in full except by Sir A. Aspinall, who has followed a slightly inaccurate copy of it made long ago by Lucas and embodied in his account of the events at Barbados. Lang does not appear to have seen this text when he wrote his article originally; and although he mentions, in a postscript, the discovery of Lucas's account by Mr. Alleyne, and notes his reference to the *European Magazine*, he assigns a wrong date to it (1814 for 1815), suggesting that he had not verified the reference personally.

I have failed to find any earlier or later reference to this case in the *European Magazine* or its contemporaries. Sir James Alexander instances it in his book (1833). The name should be Stanton—a mistake which has led astray several later inquirers, including Lang. There is no "Staunton" in Suffolk: but there is a "Stanton", about nine miles north-east of Bury St. Edmunds. Lewis's *Topograhpical Dictionary of England* (1831) gives it as then consisting of two united parishes, Stanton All Saints and Stanton St. John, which suggests a doubt as to which parish churchyard includes the vault in question. Lucas seems to have known of this, as he notes on his copy of the *European Magazine* extract: "(Qy. Which of the Stantons? N.L.)." Sir Arthur Conan Doyle states that inquiries made "from the present vicar of the parish" have not yielded any further evidence or details.

I shall return later, so far as it concerns the happenings at Barbados, to the very natural theory that these disturbances of coffins have been occasioned by water flooding the vaults.

The evidence for the second case of the kind in England is contained in a communication made to *Notes and Queries* in 1867 by Mr. F. A. Paley, of Cambridge, a well-known scholar.

"DISTURBANCE OF COFFINS IN VAULTS

"As attention has been directed to this rather curious and perhaps novel subject, I beg to add an instance which occurred within my own knowledge and recollection (some twenty years ago) in the parish of Gretford, near Stamford, a small village of which my father was the rector. Twice, if not thrice, the coffins in a vault were found on reopening it to have been disarranged. The matter excited some interest in the village at the time, and, of course, was a fertile theme for popular superstition: but I think it was hushed up out of respect to the family to whom the vault belonged.

"A leaden coffin is a very heavy thing indeed; some six men can with difficulty carry it. Whether it can float is a question not very difficult to determine. If it will, it seems a natural, indeed the only, explanation of the phenomenon, to suppose that the vault has somehow become filled with water.

"I enclose an extract from the letter of a lady to whom I wrote, not trusting my own memory, as to the details of the case.

" 'PENN., *Oct.* 15, 1867

" '. . . I remember very well the Gretford vault being opened when we were there. It was *in the church* and belonged to the . . . family. The churchwarden came to tell the rector, who went into the vault, and saw the coffins all in confusion: one little one on the top of a large one, and some tilted on one side against the wall. They were all *lead*, but of course cased in wood. The same vault had been opened once before, and was found in the same state of confusion, and set right by the

churchwarden, so that his dismay was great when he found them displaced again. We had no doubt from the situation and nature of the soil that it had been full of water during some flood which floated the coffins. I dare say . . . is still alive, and could give the date, and I almost think . . . saw what had happened. I felt no doubt myself that lead coffins could *float*. We know a large iron vessel will, without any wood casing, and I suppose the flood subsiding would move them. The vault had been walled up, so that no one could have been in it. . . .' "

The good lady's notions of hydrostatics are a little crude, but, as will be seen later, she was quite correct in supposing that a coffin could float. Mr. Paley's caution in suppressing every other name in connection with the case was perfectly proper and considerate, but is a trifle exasperating at this distance of time. It is reminiscent of Theodore Hook's parody of the style in which Moore wrote his emasculated and cryptic life of Byron:

". . . He told me one night that —— told —— that if —— would only —— him, —— she would —— without any compunction; for her ——, who though an excellent man was no ——, and that she never —— and this she told ——, and ——, as well as Lady —— herself. Byron told me this in confidence, and I may be blamed for repeating it; but —— can corroborate it if he happens not to be gone to ——."

The third case is alleged to have occurred in 1844 at Arensburg, on the island of Oesel, in the Baltic. Unfortunately, although we have a most circumstantial narrative of all that happened, or is supposed to have happened, it is at third-hand; it is the work of a man who, while honest and sincere, seems to have possessed very little power of weighing evidence; and comparatively

recent attempts to obtain corroborative evidence have only succeeded in throwing considerable doubt on the whole story.

With these reservations, I give the account, which is too interesting to omit. It was first published in Robert Dale Owen's *Footfalls on the Boundary of Another World* (1861). I have condensed it a good deal; the original (which, be it noted, forms the sole authority for the story) is rather verbose, and written in the "succulent bivalve" manner.

The story was told to Owen in Paris on May 8, 1859, by the son and daughter of one of the principal actors in it—the Baron de Guldenstubbé. The events were said to have occurred in the summer of 1844—fifteen years earlier.

The cemetery of Arensburg, the only town in Oesel, contains several private chapels, each owned by some "family of distinction", with its vault annexed. It was the custom for the coffins (of heavy oak) to be left for a time in the chapel and then transferred to the vault, where they were placed side by side on iron cross-bars.

A highway runs by the cemetery, and from this three of the chapels, facing the road, are very conspicuous. It was in one of these three chapels, belonging to the family of Buxhoewden, that the disturbances occurred; and it was, indirectly, the nearness of the road that first caused attention to be drawn to them.

On Monday, June 22, 1844, a peasant woman named Dalmann had driven to the cemetery to visit her mother's grave, which was close to the Buxhoewden chapel. She made her horse fast (presumably to some "hitching-post", or to the railings, if there were any) opposite that chapel. On coming away, she found the animal in a state of mortal terror, and went for a "vet". The latter sapiently if, cautiously, declared that the horse "must have been excessively terrified from some cause or other", and

did what he could for it. The animal, one is glad to say, recovered—the terror of dumb animals is a pitiable thing to see. A day or two later Mme Dalmann told the story to the Baron de Guldenstubbé, a member of "one of the oldest noble families of Livonia", at his château, near Arensburg. He, as was natural, treated it lightly.

The following Sunday several persons who, like Mme Dalmann, had fastened their horses up opposite the Buxhoewden chapel (presumably during service) found them trembling and sweating with terror when they returned. Suspicion focused itself on the chapel; and several people heard, or thought that they heard, rumbling noises or groans coming from it. One day in July the same thing happened again. Eleven horses had been tethered close to the chapel; some passers-by, hearing noises coming from the chapel, raised an alarm; and when the owners reached the spot, they found the horses in a dreadful state. "Several of them, in their violent efforts to escape, had thrown themselves on the ground, and lay struggling there; others were scarcely able to walk or stand; and all were violently affected, so that it became necessary immediately to resort to bleeding and other methods of relief." This, be it remembered, was in 1844, when bleeding was a very ordinary method of treatment; in fact, the sheet-anchor of most medical men. Three or four of the horses were too far gone to recover.

Their owners, quite rightly, felt that this nuisance ought to be abated. They addressed a complaint to the Consistory—an ecclesiastical court sitting periodically at Arensburg—which they apparently considered the most appropriate tribunal. But the Consistory, doubtful of its legal or material power to put things right, did, for the moment, "nothing in particular, and did it very well".

About this time there was a death in the Buxhoewden family, and during the funeral service in the chapel some

of the congregation thought that they heard groans in the vault below. This, however, was probably the effect of imagination, excited by rumour. But after the service some of the bolder spirits went down to the vault—and although they heard nothing they found that almost all of the coffins in it "had been displaced, and lay in a confused pile". They could find nothing to account for this. The vault was usually kept locked, and the locks were intact. They replaced the coffins, and locked up the vault again.

The cemetery had by now acquired rather a bad name in the neighbourhood; and as its ill-fame continued to spread the Consistory found itself obliged to do something. They proposed to make an official inquiry. The Buxhoewden family at first opposed this, on the ground that some enemy of theirs, wishing to start a scandal, was probably at the bottom of the whole thing. They came round, however, after satisfying themselves that, on the face of it, no one could have got into the vault without their knowledge. As a first step the Baron de Guldenstubbé, who was President of the Consistory, visited the vault with two of the Buxhoewdens, and found the coffins again in the same disorder. This decided matters; and after replacing the coffins an official investigation was put in hand, and a committee appointed to make it.

The committee consisted of de Guldenstubbé, as President of the Consistory; the bishop of the province, as its Vice-President; two other members of the Consistory; a doctor named Luce; Schmidt (the burgomaster) and one of the syndics, as representing the authorities of the town; and a secretary—in all eight persons.

Very naturally, they began by examining the vault. They found that all the coffins, except three, had again been displaced—the third recorded disturbance within a very short period. It may be noted that the number of coffins in the vault is not stated by Owen; but as he uses the expression "many of the coffins", and gives one to

understand that the three left undisturbed did not form a very striking exception, we may safely put the total number down at over a dozen.

The committee first looked for traces of robbery, without success. None of the ornaments of the coffins had been touched; and on opening one or two, the trinkets forming part of the funeral attire were found *in situ*. Robbers had entered a near-by vault some time before and stolen the gold-fringed velvet coffin-palls; so that it was natural, though mistaken, to suppose that robbery was at the bottom of the disturbances in the Buxhoewden vault.

The committee then took up the hypothesis which the owners of the vault had already abandoned—that some lewd fellow of the baser sort, "not having the fear of God before his eyes but rather seduced by the craft and malice of the Devil", had gone so far as to dig a tunnel, from some distant coign of vantage, underneath the chapel and into the vault. To test this not very probable theory, they employed workmen to take up the floor of the vault and examine the foundations of the chapel. No traces of tunnelling were found. One wonders that they did not, as in the Crippen case, adopt the simple plan of putting down an inch or so of water on the floor of the vault; the water would have sunk quickest, in all probability, on the line of a tunnel; and if it had remained, or sunk very slowly, it would have indicated that the vault was likely, in heavy weather, to be flooded—a theory which the committee does not seem to have considered.

It declared itself baffled for the moment. The coffins were replaced, the inner and outer doors of the vault were closed and sealed, both with the Consistory and municipal seals, and fine wood-ashes were scattered over the stairs leading from the chapel to the vault, and over the floor of the chapel. A similar layer of ashes had already been laid down in the vault itself. "Finally, guards, selected from the garrison of the town, and relieved at short

intervals, were set for three days and nights to watch the building and prevent any one from approaching it."

At the end of the three days the committee returned to the charge, and had the vault reopened. In view of the very short interval, it would not have been surprising had they found everything as they left it—whatever its state might have been had the opening been postponed, say, for some months. But the vault, as if flattered by the attention paid it by such dignified bodies as the Consistory and the municipality, rose most punctually to the occasion.

> "Both doors were found securely locked and the seals inviolate. They entered. The coating of ashes still presented a smooth, unbroken surface. Neither in the chapel nor on the stairway leading to the vault was there the trace of a foot-step, of man or animal. The vault was sufficiently lighted from the chapel to make every object distinctly visible. They descended. With beating hearts they gazed on the spectacle before them. Not only was every coffin, with the same three exceptions as before, displaced, and the whole scattered in confusion over the place, but many of them, weighty as they were, had been set on end, so that the head of the corpse was downward. Nor was even this all. The lid of one coffin had been partially forced open, and there projected the shrivelled right arm of the corpse it contained . . . (that of one of the Buxhoewdens who had committed suicide)."

The committee satisfied themselves, as on the former occasion, that no robbery had been committed, and that there was no secret entrance to the vault; and then, as at Barbados, the coffins were removed, and buried elsewhere.

Owen concludes his story with the following important passage:

> "An official report . . . was made out by the Baron de Guldenstubbé, as president, and signed by himself,

by the bishop, the burgomeister, the physician, and the other members of the commission, as witnesses. This document, placed on record with the other proceedings of the Consistory, is to be found among its archives, and may be examined by any traveller, respectably recommended, on application to its secretary."

In view of this very categorical statement, the sequel is unfortunate.

In 1899, in the course of some correspondence on the subject of "The Poltergeist" in the *Journal of the Society for Psychical Research*, the late Dr. Alfred Russel Wallace quoted Owen's account of the disturbances at Arensburg, and spoke of it as ". . . the best evidence as to occurrences which were, and are, wholly inexplicable".[1] He elicited a rather disquieting rejoinder from the late Frank Podmore.

"(7) THE DISTURBANCES AT ARENSBURG.—As Dr. Wallace tells us, the facts were communicated[2] to Dale Owen by Mlle. de Guldenstubbé and her brother. They had heard them from the late Baron. The account, as it reaches us, is therefore third-hand. Neither Dale Owen nor his informants profess to have seen the documents which constitute the strength of the evidence. Until we have a certified copy of those documents, the case, I submit, is not before the court."[3]

Podmore's scepticism was quite justifiable; particularly in view of what, I believe, is the rule of the S.P.R., that save in very exceptional circumstances it will not accept evidence at second and remoter hands.

[1] *Journal of the Society for Psychical Research*, (February) 1899, pp. 28, 29.

[2] "Not, it is to be presumed, in writing: Dale Owen's words are, 'the facts above narrated were detailed to me', etc." [Note by Podmore.]

[3] *Journal of the S.P.R.*, (June) 1899, p. 93.

The question thus raised in 1899 waited for its answer until 1907, when the *Journal of the S.P.R.* published the results of some inquiries made by Count Perovsky Petrovo-Solovovo.[1]

The Consistory of Arensburg, in which the all-important documents were said by Owen to have been deposited, was closed in 1889 as the result of the ecclesiastical reforms initiated by the Tsar Alexander III. Its archives were transferred to the "Livländisches Evangelisches-Lutherische Consistorium" at Riga. Solovo applied to this body for information, and received the reply that the Oesel archives in their custody contained no documents bearing on the subject; they referred him to the archives of the Church of St. Laurentius at Arensburg. A letter addressed there brought a reply from the Rev. Lemm, "Ober-pastor" of the church in question, "which was wholly negative". Mr. Lemm also mentioned that, some years before, the present Baron Buxhoewden, who had seen an account of the 1844 disturbances in a Warsaw newspaper, had communicated with him on the subject, but had failed to find any further information either at Arensburg or at Riga.

It would seem, then, that the document paraded with such a flourish of trumpets has, if it ever existed, vanished into thin air; while both its existence and the facts of Owen's narrative are rendered, to say the least, doubtful. But the proof is not quite conclusive. In spite of the unsuccessful searches, it is quite possible that the document still exists.

Benjamin Franklin once delivered himself of the excellent aphorism, "If you want a thing done, go: if you don't, send." A search made by a third party, at a distance, for documents with whose special importance he is not personally impressed, is rarely satisfactory. An experience of my own is a case in point.

Some years ago, while compiling a book on the marine

[1] *Journal of the S.P.R.*, 1907–8, pp.30, 128, 144, 158–60.

chronometer, I came across a statement by Sir George Airy, Astronomer Royal, in his Annual Report for 1841, that a set of elaborate drawings of John Harrison's first three marine timekeepers had been transmitted to the Observatory by Messrs. Arnold and Dent, and placed in safe custody there. I very much wanted to inspect those drawings; and from what I knew of Airy's elaborate system of filing and indexing every scrap of paper that passed through his hands, I thought it would be no very hard job to find them. The Observatory authorities, with their customary kindness, made a search—which drew blank. Of the drawings, not a trace could be found—only a letter written by Airy to the Admiralty, stating that they had been received and that "every care will be taken of them". As a sarcastic, if unintentional, comment on the efficiency of his methods of filing papers, this would be hard to beat.

Luckily, however, I persisted. It would have been unreasonable to expect the Observatory authorities, who had already spent a good deal of valuable time over the matter, to make a further search. But I obtained their permission (a privilege which I appreciated) to make a personal search in their archives; and, after a long hunt in all likely and unlikely places, I unearthed (in one of the latter, needless to say) the missing drawings, securely hidden at the end of a volume of miscellaneous papers.

But if Solovovo's inquiries as to the documentary evidence mentioned by Owen resulted in casting some doubt (but scarcely absolute discredit) upon the latter's story, support was, at the same time, lent to it by a letter to Solovovo from "a leading member of the Buxhoewden family"—probably the "present Baron Buxhoewden" mentioned by Mr. Lemm. The letter, written from Arensburg, stated that the writer—aware that the archives of the Arensburg Consistory had been removed to Riga, and that there was nothing (officially) to be found there—had visited Oesel, and examined the archives of the former "Landgericht",

but had not found, at present, anything bearing on the matter. It added, however (I translate from the original French):

> ". . . All of the old persons whom I have questioned upon the subject remember the incident in question perfectly, and the greater number of them affirm that they have heard it said that an official report (*procès-verbal*) was drawn up. With a few exception, the details mentioned in your English journal have been entirely confirmed, but, to my great regret, it has been impossible for me to discover a trace of the official report."

Although this is only hearsay evidence, it suggests at least that Owen was not the victim of an elaborate practical joke on the part of the two Guldenstubbés. It is possible, but not very probable, that the report in question may still be brought to light.

The letter also indicates that the Buxhoewden family considered the disturbances to have been caused by water making its way into the vault—a point which will shortly be discussed.

Such is the synopsis of the evidence for the cases, apart from that at Barbados, in which coffins have been found disturbed by some recondite cause. All four cases are curiously similar in outline, and I propose, while not asserting that any one explanation is equally applicable to all of them, to use the other three as a basis of comparison while discussing the explanations which have been put forward in connection with the disturbances at Barbados, which I consider the best-authenticated and, if the expression be permitted, most inexplicable case of the four.

Whatever may be thought of the evidence in the three other cases, particularly that at Oesel, I imagine that few will deny the actual fact of the coffins at Barbados having repeatedly been found disturbed, and the equally certain

fact that the witnesses to the occurrence were completely puzzled to account for it.

Starting here, it may be premised that the agency producing the disturbances was either

1. Natural, or
2. Human, or
3. Neither human nor natural.

And, like the negro preacher who announced that he would divide his sermon into two parts, I propose to "wrastle with de easy part first". We have no right to postulate the existence of any agency of the third order unless and until discussion of the other two has failed to throw light on the matter.

Of natural causes, those which have been suggested are

(*a*) Earthquakes.
(*b*) Gases escaping from the coffins.
(*c*) Flooding of the vaults by casual water, which has afterwards subsided.

This classification appears to be exhaustive.[1]

The West Indies are undoubtedly subject to earthquakes; it may be remarked in passing that this cannot be said, in the same degree, of either England or the island of Oesel. But an earthquake so extraordinarily limited in scope as to produce violent motion in an area 12 feet by 6 feet, without the slightest tremor being felt elsewhere, is unknown to seismology; while to imagine that such an earthquake could occur repeatedly in exactly the same spot is an abuse of one's intelligence.

The gas theory I have mentioned only for the sake of completeness. There is no need to discuss it in detail.

[1] I exclude, as obviously failing to cover the facts, two other suggestions—(*d*) animals entering the vault, and (*e*) the action of lightning—which have reached me from various correspondents.

I will only say that no volume of gas that could conceivably be generated would be able, even if it escaped suddenly at a single small vent, to produce any motion of the coffin in which it had been confined; unless, indeed, the latter were balanced on knife-edges or floating in water, or otherwise relieved of the effects of weight and friction. As to its floating in water, it will be seen that this supposition is inconsistent with the assumption of gas escaping from the coffin.

The supposition that the disturbance of the coffins was due to intermittent flooding of the vault has far more to be said in its favour; in fact, it is one which, *on the evidence*, completely explains the occurrences at Stanton and at Gretford. I do not think, however, that it is applicable in the Oesel case (upon which, however, I do not much rely), and I am quite certain that it does not explain the events at Barbados.

Let us see what is to be said in its favour.

In the first place, Mr. Paley's correspondent was perfectly right when she asserted her belief that the coffins in a flooded vault, if freely laid therein, would float. They would undoubtedly do this *so long as they were watertight* (or, which is the same thing, gastight). This can be proved both by calculation and by direct evidence.

A coffin of ordinary size (actually, the size does not affect the question) and constructed in the English fashion, with a wooden shell, an inner lead casing and a heavy outer shell of wood, weighs in all about 8 cwt., and would displace, if wholly immersed in water, some 18 cubic feet. The weight of this displaced water, assuming it were fresh (if it were salt, the buoyancy of the coffin would be increased about 2½ per cent) would be about 10 cwt. Allowing 1½ cwt. for the body, the coffin would still just float with a 50-lb. weight attached to it.

The following extract from the *London Evening Post* for May 16, 1751, provides excellent direct evidence on the point:

"We have an account from Hambourg that on the 16th April last, about six leagues off the North Foreland, Captain Wyrck Pietersen, commander of the ship called the *Johannes*, took up a coffin made in the English manner, and with the following inscription upon a silver plate, 'Mr. Francis Humphrey Merrydith, died 25 March, 1751, aged 51', which coffin the said captain carried to Hambourg, and then opened it, in which was enclosed a leaden one, and the body of an elderly man embalmed and dressed in fine linen. This is the corpse that was buried in the Goodwin Sands a few weeks ago, according to the Will of the deceased."

No doubt the coffin was buried at low water, when the sands were uncovered, and was freed by the scour of the tide. But its buoyancy must have been more than a little to extricate it entirely, for the Goodwins are proverbially tenacious of anything that comes into their keeping.

The Times of August 3, 1907, reports a sitting of the Consistory court of London, at which a petition for a faculty was presented by the vicar and churchwardens of Edgware parish church. It was desired to remove various remains buried in the church itself, in a series of brick graves under the aisle and the transepts, covered with large flagstones. It had been found that these graves were full of water to a depth of from 4 to 6 feet, in which some of the coffins, and also decayed bones, were floating.

The Edgware case is, if anything, more significant than that of the coffin found floating at sea, for two reasons. The floating took place in fresh water, not salt; and the coffins, although much older, had obviously remained watertight. It may be pointed out that a non-watertight coffin of ordinary pattern would certainly not float in water.

It is also an excellent proof, if any were needed, that vaults do sometimes become flooded. And it will be conceded that if this occurred in any of the cases I am

considering, the result would necessarily be some disturbance of the coffins. Even if the rise and fall of the water were very gradual, there would probably be some slight currents and eddies, and it is too much to expect that the coffins should all come down again exactly in the positions from which they rose. If some floated and some remained submerged, it would be about an even chance that, when the water sank, one or more of the floating coffins would subside across the lower ones, in an unstable position, and so be found tilted, or even bottom upwards. Even if all floated, they might not necessarily be all found side by side on the floor.

The theory put forward by the Buxhoewden family, in the letter previously mentioned, takes this factor into account. Shortly, it was stated that Oesel is subject to periodical accumulations of surface water (*Grundwasser*), which are common in the autumn and spring, and rise, at those periods, almost to ground-level in some parts of the island, only to disappear very rapidly in the summer and the winter. Underground tombs, in such circumstances, become tanks. The letter adds that other tombs have since been found, occasionally, to be full of water, in which the coffins were floating—some of them, singularly enough, head downwards.

This is not impossible. If the coffin were only just buoyant, it would have a negligible "metacentric height", and would tend to turn into its most stable position, whatever that might be. As compared with a coffin of considerable buoyancy, which would undoubtedly float in a horizontal position, the stability of such a coffin would be analogous to that of a submarine, awash, contrasted with that of a surface craft; and it is well known that submarines have no great stability. When one reflects that the head is, bulk for bulk, by far the heaviest part of the body, it is not at all wonderful that some coffins might, if the depth of water allowed, float head downwards, and retain that position when the water in the vault subsided.

Again, it is by no means certain that water entering a vault would leave any very obvious traces, provided that the inundation did not last very long. Most vaults are more or less damp, so that traces of extra damp would not be very obvious. Readers of that classic of Bacchanalian literature, Professor Saintsbury's *Notes on a Cellar-book*, will remember his account of the cellar which, "at certain times of the year, used to be filled about a foot deep with the most pellucid water, apparently rising from the earth. This remained some time, and disappeared as it came." It does not seem to have done any great harm, and it certainly did not prevent the use of the adjoining cellar as a repository for nobler liquids.

It does not even seem likely that traces of the flooding would be found upon the sand or ashes covering the floor; the effect of the water on this, if gradual, would probably be to make it smoother than before.

The flooding theory, then, has much to be said in its favour. It provides a natural explanation of the whole mystery; it produces an agency of quite sufficient power to displace the coffins, overturn them, and even stand them head downwards; and it does not involve any disarrangement either of seals or private marks on the outside of the vault or of the material strewn on the floor for the purpose of detecting foot-marks. In the Stanton and Gretford cases there seems to be no valid reason for declining to ccept it; although there might be such reason if we knew more of these two rather obscure and vaguely documented cases.

But I cannot think that it entirely explains the disturbances at Oesel; and it is quite inapplicable in the case of the Barbados vault.

At Oesel we have to assume that the flood-water rose from well below the floor-level of the vault to a height of at least 6 feet above it (as shown by the inverted coffins) and again sank, so as to disappear completely, within the period of a few days. Not only must it have done this,

but it must have done it again and again, at quite short intervals, at least four times. In the case of the official investigation, the whole operation must have been performed in three days only; and I cannot conceive any hypothesis which would make this rapid flux and reflux even plausible. In addition, the coffins would certainly not have had time to dry, and, in common with anything of perishable material, such as a pall, etc., left in the vault, they would have exhibited most obvious traces of damp.

The same reasoning applies, although not with so much force, to the Barbados case. The intervals were longer; but one was of a month only, and in the early autumn. But there are other and much more cogent objections.

In the first place, the churchyard stands on a headland overlooking Oistin's Bay, a situation not very favourable to any accumulation of surface water. On the other hand, the vault is partly dug out of the limestone rock, in which the presence of springs is not impossible.

But the possibility of the disturbances having been due to the action of water was not overlooked at the time, as at Oesel. Lucas, in his account, is specific as to the point:

> "There was no vestige of water to be discovered in the vault; no marks where it had been; and the vault is in a level churchyard, by no means in a fall, much less in a run, of water."

Just previously he has remarked:

> "Why were the coffins of wood *in situ*? and why was the bundle of Mrs. Goddard's decayed coffin found where it had been left? Wood certainly would first float."

His second question is the crux of the matter. We have the written testimony of two eye-witnesses, Lucas and Orderson, that the remains of Mrs. Goddard's wooden

coffin, tied together in a bundle, were found undisturbed when the vault was last opened. Unless we disregard this testimony altogether—in which case we might as well disbelieve the whole story and be done with it—this seems quite conclusive evidence that whatever agency may have caused the disturbances, it was not water. If the flood were deep enough to float the coffins, it must, *a fortiori*, have floated the bundle—and, short of a miracle, this could not afterwards have been found undisturbed. It may also be pointed out that all but two feet of the vault was above the ground-level, so that it is difficult to imagine it being flooded to a greater depth—while two coffins of ordinary height lying one on top of the other would not be floated by two feet of water.

There arises, in consequence, the question whether the disturbances were the work of some human agency. Three other questions at once suggest themselves, assuming this to be the fact:

1. Who did it?
2. Why was it done?
3. How was it done?

To the first two questions there are several possible answers, but they can only be the merest guesses. The disturbances began when a member of the Chase family was buried in the vault; and Thomas Chase, the head of the family, who shortly followed there, seems to have been a man of strong and possibly cruel character—one who might easily make bitter and vengeful enemies, particularly among his negro dependants. As against this, one imagines that nothing but the most powerful inducements could have prevailed upon a superstitious negro to break into a vault, probably by night; although there is a case on record of an occurence almost as unlikely, when a negro broke *into* Dartmoor prison, having an account to settle (by means of a razor, or other convenient implement) with the

chief warder—an errand happily bootless in more senses than one.[1]

Admittedly, the Barbados story has a hint of Voodooism, which would at once have been suspected had the scene been laid in Hayti; but if we concede, for the moment, that some negroes or other enemies of the Chases had made their way into the vault, it is difficult to imagine why they (there must have been more than one) should have contented themselves with merely throwing the coffins about, and not have attempted to open them for the purpose of mutilating or defiling their occupants. They must, on this supposition, have had several opportunities of doing so.

It is true that, as far as we know, the coffins, once deposited, were never opened for examination, and it is therefore a matter of inference that their contents went unmolested: but they were undoubtedly scrutinized, and we can at least conclude that they showed no external signs of having been opened, and that their weight was not appreciably altered.

But to the third question—how was it done?—there can scarcely be any answer. In view of the repeated examinations of the walls, sides, and floor of the vault, and the precautions taken, it seems impossible during the period when Combermere's seal was on the slab, and most extremely unlikely at earlier dates, that any one, even if they had the will, could have had the power to enter and leave the vault undetected.

If, then, natural causes and human agency must both be rejected as an explanation of the disturbances at Barbados, what is left? We must conclude, I submit, that the agency producing the disturbances at Barbados was neither human nor natural. As to its nature, apart from this

[1] The story is told by Sir Basil Thomson, in his book *The Criminal* (London, 1925, pp. 129, 130). The negro (a discharged convict) had walked to Dartmoor from London with the intention of killing the chief warder and setting fire to the prison. He made his attempt on the night of August 17, 1890.

combination of negatives, I do not feel called upon to offer any opinion—which, after all, would be of little value.

Others, however, such as Dale Owen and the late Sir Arthur Conan Doyle, have been less Pyrrhonic. The latter, in particular, who had no doubt as to the psychic nature of the phenomena, has reached three principal conclusions, which I will summarize briefly but, I hope, fairly.

1. That the disturbances were the work of forces desiring the more speedy decomposition of the bodies. It is claimed that this explained the particular animus directed against those with leaden casings, while the wooden ones were left undisturbed; and that the desired result was achieved, in that the coffins were ultimately buried elsewhere.
2. That the physical force necessary to move the coffins was derived in some manner from the "effluvia" of the overheated negroes employed in carrying the coffins; which "effluvia" were necessarily retained in the confined space of the hermetically sealed vault.
3. That the disturbances were facilitated, or even occasioned, by the presence in the vault of the corpses of two persons who had committed suicide. "There is some evidence . . . that when a life has been cut short before it has reached its God-appointed term, whether the cause be murder or suicide . . . there remains a store of unused vitality which may, where the circumstances are favourable, work itself off in capricious and irregular ways. This is, I admit, a provisional theory, but it has been forced upon my mind by many considerations. . . ."

Dale Owen, reaching the same conclusion as to the "spiritualistic" character of the disturbances at Oesel (he does not seem to have known of any of the other cases),

goes no farther than to suggest that their *raison d'être* may have been the (alleged) conversion of an infidel member of the committee at Arensburg, one Dr. Luce—a pennyworth of sack to an intolerable deal of bread. But Sir Arthur Conan Doyle's views, however far-fetched they may appear to ordinarily minded people (and as, I admit, they seem to myself), certainly deserve careful examination.

As to the actual fact of wooden coffins in the vault at Barbados having been left undisturbed while the leaden ones were scattered, the evidence is somewhat conflicting. There are but two stated to have been of wood only; those of Mrs. Goddard (the first occupant of the vault) and of Thomazina Clarke (the last person buried there); the Alpha and Omega of the inmates.

Turning to the four MS. authorities, "A", "O", "B", and the Lucas-Orderson version, I find that "A" and "O" agree in two statements.

> "(July 6, 1812) Dorcas Chase buried. The two other coffins were in their proper places. They were leaden coffins.
>
> "(August 9, 1812) Hon. Thomas Chase buried. The two leaden coffins were found out of place."

The Christ Church burial register supports these statements. "B" omits any mention of Mrs. Goddard's interment, and accordingly makes no specific reference of any kind to her coffin. The Lucas-Orderson version agrees with "A" and "O" in saying that when Dorcas Chase was buried "the two *leaden* coffins" were displaced.

Now when Dorcas Chase was buried, there were only two other coffins in the vault, those of Mrs. Goddard and M. A. M. Chase. On the evidence of the entries relating to Dorcas Chase's burial, it most certainly appears that Mrs. Goddard's coffin was a leaden one.

On the other hand the entries relating to the burial of Thomas Chase suggest equally strongly that the third

coffin then in the vault—Mrs. Goddard's—was undisturbed, and was of wood. This is also confirmed, at first sight, by the statement in the Lucas-Orderson version about the remains of Mrs. Goddard's coffin having been tied up in a bundle. If her coffin had been of English pattern one could reconcile the discrepancies by supposing that the outer wooden casket had decayed and come away, converting the coffin to a leaden one of Barbados type, which would have taken its place with the others and so accounted for the silence on the subject of what happened to her body—one of the most remarkable features of the whole story. But there is a strong objection to this theory, for none of the sketches depicting the state of the vault at the last two openings show seven coffins.

I must leave the question of Mrs. Goddard's coffin, noting that while it *may* have been of wood only, the evidence for this is not very satisfactory.

There seems to be no doubt that Thomazina Clarke's coffin was of wood; whether this had a lead lining does not appear. But there is still a difficulty, for it seems to be uncertain whether this coffin was disturbed or not. It was the last buried, so that we must look to the narratives of the final opening of the vault for information.

The authorities are equally divided on the subject. "A" is not specific on the point, but allows it to be inferred that the Clarke coffin was displaced along with the others. "B" does the same. The sketches accompanying "A" (for which I have already indicated my own preference) support this view.

Not having been able to consult "O", I am unable to say whether it remarks specifically on the point. Lang says, "The sketches given by O vary much from A." I infer that the sketches accompanying "O" probably agree with those in Lucas's statement.

The latter does not say in what state any of the coffins were found; it merely refers to "the annexed drawing". In this, the Clarke coffin is shown as undisturbed, and that

of Samuel Brewster also. This fact casts some doubt on the authenticity of the drawing. Such a striking exception as two coffins left untouched out of six would surely, one thinks, have been commented on in the other narratives.

The balance of evidence, I suggest, is in favour of the view that the Clarke coffin was disturbed. Summarizing, the case for supposing that wooden coffins were immune from the forces disturbing those of lead does not appear to be at all clearly made out.

I turn to Sir Arthur Conan Doyle's second conclusion.

I am far from denying that the "effluvium emitted by an overheated negro is very strong indeed";[1] but I find it difficult to believe that the concentrated aroma of even half a dozen buck-niggers could, unaided, shift a coffin whose weight was a good load for their united arm-muscles —or, for that matter, disturb even a much lighter object. Sir Arthur suggests that these "effluvia", under the more dignified style of "emanations of the living", were concentrated in the vault as in the cabinet of "a genuine medium", and used (apparently by its inmates, or some of them) in the same manner as that practised by the medium, whatever that may be.

On this I am not competent to pronounce. I have never met a genuine medium—on the other hand, I have made no great efforts to do so, although from my casual encounters with mediums of the other kind I should imagine that the earnest searcher would do well to equip himself, after the manner of Diogenes, with a powerful lantern and a stout pair of shoes.

On the question of the power, if any, permitted to suicides every one must judge for himself. There is this to be said *in limine*, that Reece (writing in 1864) states that both Thomas and Dorcas Chase died by their own hand, the daughter having starved herself to death owing to her father's cruelty, "wherefore the other corpses were desirous

[1] The negro says the same thing of the white man. ". . . *cuiusque stercus sibi bene olet.*"

to expel her". And, whatever we may think of the truth of the almost innumerable recorded cases of supernatural phenomena, it is undeniable that a connecting thread links the bulk of them together; they are almost all associated with persons who have undergone strong emotions, often terminated by a violent and/or premature death. Those who die peacefully in their beds at the close of the three-score years and ten are not often alleged to return as ghosts—that appears to be the privilege of the victim of accident or murder, the suicide, the revengeful and the remorseful. In the common phrase, then, "there is something to be said" for the idea that the presence of two suicides in the vault at Barbados "might have something to do with" the disturbances there.

I do not accept this view. But if I reject it, it is not on the ground that I am an absolute disbeliever in all forms of "supernatural" phenomena—on that subject I neither affirm nor deny anything. My reason for rejecting it may irritate the "unco' guid", but I should think myself wanting in common honesty if I omitted to state it.

We have gone a long way from the days when, as Hood puts it,

> . . . They buried Ben in four cross-roads,
> With a *stake* in his inside,

but even in those days there was a queer refinement of kindliness and charity in that apparently barbarous treatment. The stake, it is true, was intended to ensure that the suicide did not haunt the neighbourhood—did not, as they still say in Rumania, become Un-Dead. But the burial at the cross-roads served a more noble purpose. As the rubric said, and still says, " . . . *the Office ensuing is not to be used for any that . . . have laid violent hands upon themselves*". But the suicide, if huddled by night into the ground at the cross-roads, might still, even though he were denied Christian burial, have the cross over his grave.

Nowadays we are more charitable. In default, and sometimes in defiance, of the clearest evidence coroner's juries find, in their verdicts, that "temporary insanity" which averts the angers of the Church. And if the latter still formally records its readiness to cast the first stone, its practice is uniformly more kindly and, I suggest, more Christian.

Putting aside those well-known cases in which every man of common sense will agree that suicide was not only justifiable but even a duty—such as the case of that "very gallant gentleman" Captain Oates—there is much to be said for the view put forward by Winslow in his *Anatomy of Suicide*,[1] that *all* suicides are, in effect, insane when they take their lives. And when we reflect on the anguish of mind which they must have undergone, and the agonies of physical pain which they often undergo in struggling to cut the Gordian knot of life and troubles simultaneously, it is difficult—and not only is it difficult, but it is harsh and callous—to shut out of one's mind the belief, or at least the hope, that they have already been punished enough, and may now take their rest.

The Gods may release
That they held fast.
Thy soul shall have ease
In thy limbs at the last.
But what shall they give thee for life, sweet life that is overpast?

If we come to be judged, assuredly it will not be upon the circumstances of our deaths alone, but upon the fabric and pattern of our whole lives. And if we ourselves are charitable to the suicide, and if we look for a larger charity elsewhere, what right have we to assume that the dead—sinners like ourselves—would, if they could, be less charitable?

[1] *The Anatomy of Suicide.* Forbes Winslow, M.R.C.S. (London, 1840, pp. 221–45.)

III

THE WIZARD OF MAURITIUS

MANY years ago, in

> . . . times when I remember to have been
> Joyful and free from blame,

I came across a copy of Sir David Brewster's *Letters on Natural Magic*. It seemed to me then, and it seems to me now, one of the most wonderful books ever written. With advancing and sorrowful years, I have come to know more about Brewster. I am better able to appraise at its true value, for example, that laboured feat of idolatry which he called *The Life of Sir Isaac Newton*, and which, if touched with Ithuriel's spear, must have displayed on the covers of both its portly volumes the legend, "An Opinionated Defence of the Absolute Uprightness, Mental Chastity, and Fair-Mindedness of Sir Isaac Newton against the Clearest Evidence". But although I speedily became aware that Brewster was far more to be respected as the inventor of the kaleidoscope than as a narrator of facts, and although I regretfully decided that in the case of D. D. Home the honours rested with the American "spiritualist" and not at all with the scientist who explicitly asserted, in a private and long-unpublished letter, statements in flat contradiction to others which he soon afterwards communicated to the Press, I have never swerved in my allegiance to his amazing collection of scientific marvels.

It is a wonderful book—wonderful both for its matter and its manner. Brewster does not smack you on the back and invite you to laugh at the exploded chimeras which delighted and puzzled our ancestors; nor does he come up to you, with a twitch at your sleeve and a sneer on his lips and tell you what a fine fellow you are, to be sure, to scoff

at anything which Modern Science (always in capitals) cannot explain to your complete satisfaction. He goes to work soberly, honestly, and painstakingly to show you the *raison d'être* of the wonders he recounts, and, if he has a fault, it is one which, in all probability, arises simply from the time which has elapsed since his book was first published. Every now and again he slips in some reference to a matter which, in the first half of the nineteenth century, was probably a thing of common knowledge, but which perplexes us of the twentieth not a little.

I can scarcely give a better illustration of my meaning than the following extract, premising that Brewster is speaking of mirages, and other natural optical illusions:

> ". . . The representation of ships in the air by unequal refraction has no doubt given rise in early times to those superstitions which have prevailed in different countries respecting 'phantom ships', as Mr. Washington Irving calls them, which always sail in the eye of the wind, and plough their way through the smooth sea, when there is not a breath of wind upon its surface. In his beautiful story of the storm ship, which makes its way up the Hudson against wind and tide, this elegant writer has finely embodied one of the most interesting superstitions of the early American colonists. The Flying Dutchman had in all probability a similar origin, *and the wizard beacon-keeper of the Isle of France, who saw in the air the vessels bound to the island long before they appeared in the offing, must have derived his power from a diligent observation of the phenomena of nature.*"

As I have since learned, there can be no doubt that, when Brewster wrote the singular concluding words which I have italicized, he was referring to a subject which was, for the intelligentsia of his day, a matter of common notoriety. But, at the time when I first read them, I do not think that there were many people alive who could have made head

or tail of them. If I am bold enough to think that I have co-opted myself *in nostro docto corpore*, it is because I have since, in the course of a desultory and haphazard process of reading, conducted (to my shame be it written) not entirely apart from Government time, come across some long-forgotten papers which throw a flood of light on a very dark subject.

Broadly speaking, the evidence which I have found shows quite clearly that Brewster was well within the truth when he spoke of a man at Mauritius who could foretell the arrival of ships at the island long before they appeared over the horizon. He might have gone further, and told how this man could determine, by his mysterious gift, whether one ship, or more, was approaching, and how long it would be before it, or they, came in sight—and even state in the same manner, when on board a ship, the distance and bearing of the land when it lay a long way below the visible horizon. He might have said all this and more, and yet never have departed by one iota from the exact and certified truth.

Of all the remarkable events in connection with the wizard of Mauritius, none, I think, is more remarkable than the extraordinary manner in which some account of his doings has been preserved to our own time. The facts are as follows:

The hero of our story is an obscure Frenchman named Bottineau, who returned from the Isle de France (Mauritius) to his native country in 1784—only to find that France, in travail with an upheaval which was shortly to change the whole face of Europe, had little inclination, and less leisure, to consider or reward his newly discovered art of "Nauscopie"—the discovering of ships, when below the horizon, by means of the effect which their motion produced upon the atmosphere. Bottineau, a shadowy figure who fades out of sight almost before one has time to appraise him, seems to have made but one convert, or semi-convert, of note—the famous or infamous Jean Paul

Marat, M.D. of St. Andrew's University, and sometime a troglodytic inhabitant of the Paris sewers; but latterly, until very properly stabbed in his bath[1] by Charlotte Corday after his forensic defeat of the Girondins, one of the three most powerful men of the Terror. Time enough has gone by to forgive Marat for his crimes—the Cleon of his age, he is no more to be blamed for seizing power when his chance came than a clot of scum is for riding bravely down the stream when once it had been dislodged from its native fastness—and we must at least be thankful that a happy chance has embalmed Bottineau in his correspondence like a fly in amber.

In the year 1806 an English gentleman, whose name, unfortunately, has not been preserved, was residing, on parole,[2] at Brussels. He seems to have made the acquaintance of Madame Guilleminot, sister-in-law to the General of that name. This lady, being (in common with most French women of the time) smitten with the prevailing craze for collecting autographs, applied to one of Napoleon's numerous and virtuous sisters for a few signatures of celebrated men. The Princess in question forwarded the request to Cambacérès, then Chancellor of the Empire, at whose direction "une énorme tas de papières" was forwarded to Mme Guilleminot. It is worth while noting that many of these papers had never passed through the post office: they were original drafts, often disfigured by erasures and interlineations. This, in itself, is no marvel. During the First Empire, and still more during the

[1] As Mr. A. Neil Lyons has sung:

. . . theer 'e sot a	Joe Golightly
spettin' op the	very rightly
purple blood	knifed 'im in
like wine, cos	the spine, 'e
Joe Golightly,	sadly sot a
Joe Golightly	spettin' op the
stabbed 'im in	purple blood
the spine. When	like wine . . . (*da capo*).

[2] He was a *détenu*: one of the several thousand English people who visited France in 1802 (during the Peace of Amiens), and of whom the survivors returned to England in 1814.

Revolution itself, your private papers and, *a fortiori*, your correspondence, were never regarded as anything more than the raw material of a prosecution; and the "Cabinet Noir" must have accumulated, between 1793 and 1814, sufficient *pièces incriminatoires* to carpet the entire surface of the habitable globe.[1] What is remarkable is that, among the papers which Mme Guilleminot received, and which she gave to the *dètenu* for perusal, were some documents relating to Bottineau and the marvels which he had accomplished at Mauritius.

At this point, unfortunately, there is *hiatus valde deflendus* in our history. All that can safely be asserted is that, in some way or another, the papers in question came, in 1834, into the hands of Captain A. B. Becher, R.N., founder and first editor of that estimable monthly *The Nautical Magazine*. Any one who doubts my veracity may refer, in the Admiralty Library or elsewhere, to the issue of that periodical which, adorned on its cover by a magnificent steel-engraving of His respected (if sub-normal) Majesty King William IV of happy memory, appeared in March, 1834. There, under the suggestive heading "Original Papers" (and what paper, surely, could be more original than the one in question?), they will find, distinguished by the sub-title "The Art of Discovering the Representation of Ships, etc, in the Atmosphere", the raw material of the present essay.

One conceives the meticulous and be-whiskered Becher, type and pattern of all succeeding Assistant Hydrographers, undergoing some conflict of mind before deciding to open his columns to the amazing narrative which had fallen into his hands. His venture, *The Nautical Magazine* (still flourishing, but not conducted quite on its original lines), was only two years old. A breath could make it—or unmake it. To his eternal credit, he took the risk: and his foresight was, if anything, justified only too abundantly.

[1] I gather from a recent correspondence in *The Times* that this property is equally characteristic of disused safety-razor blades.

Not one of his readers, so far as I can discover, troubled to inquire either as to the source of his materials or as to the authenticity of the facts there embodied.

One wonders, if Becher had possessed that bent for mystification which produced Locke's *Moon Hoax*, and Poe's *Hans Pfaal*, *Von Kempelen*, and *Balloon Hoax*—all published, in reputable journals, as matters of sober fact—how far, given his obviously trustful clientèle, he could safely have gone. Surely, there never was a stranger tale than Bottineau had to tell; and, although I believe it to be strictly true, I am still perplexed to understand how his memory (he had long been dead when his narrative saw the light) managed to escape the Jeddart justice which an incredulous public meted out to Marco Polo, Mendez Pinto, du Chaillu, and a hundred others who, foolishly handicapped by veracity, have told it truths which it was not prepared to hear.

The first letter printed by Becher is one from Marat to a Mr. Daly.[1] It is not dated, but was probably written about 1785.

> "You know, my dear friend, that much of my time has lately been taken up in preparing my work upon Light, Fire, etc., for the Press: it is, however, nearly completed; you may, therefore, expect to hear from me very regularly in future. M. Bottineau, whom I mentioned to you in my last letter, has experienced every kind of disappointment. If he is able to raise sufficient money he purposes visiting London very shortly, where he is likely to meet with more success; for you gentlemen of the British Isles will, I am convinced, patronize the discovery which my friend has made. I, who have made a study of optics, meteors, etc., am, I must confess, somewhat sceptical respecting the science which he terms *Nauscopie*, or the art of discovering vessels and lands at a considerable distance: but the concurring testimony of hundreds of persons, the certificates which he

[1] I have not been able to identify Daly with any certainty.

has obtained from officers of high rank, all tend to show that there must be truth in his statement; and although he may have been neglected in France, I hope, for the honour of science, that a fair trial will be given him in your country, and that he will not be treated as a visionary.

"Certain it is, that if his art should prove to be true, incalculable advantages will be derived from it. I have seen an officer who resided six years in the Isle of France, and he assures me that the whole population will corroborate the averments made by M. Bottineau. . . .

"Such, my dear friend, is the account which M. Bottineau has given me; he has also explained the phenomenon, which, he assures me, in order to understand perfectly, only requires being on the seashore for a few hours, and that in less than a week I should understand his art as well as himself. As my poor friend looks very ill, I am afraid he will not be able to visit England, the only resource, he says, that is left to him. Mr. Moore, who has been studying medicine here for some time, leaves Paris this evening for London, and will take charge of this letter.

"I have not time to explain to you the phenomenon perceived in the atmosphere when a vessel approaches land, etc., but I will give you all the explanation in my power in my next letter, and very possibly it may enable you, who have so many opportunities of visiting the coast, to ascertain whether the art of *Nauscopie* be one of those sublime discoveries that do honour to the genius of man. For myself, if I could conveniently visit the seashore, I would certainly make more than one trial. When I have sent you the explanation, you will judge for yourself; and do not act as the Abbé Fontenay, for one of your poets has said wisely, that 'There are more things in heaven and earth than are dreamt in our philosophy'. Adieu.

"MARAT"[1]

[1] In those days, as now, it was a common French practice to sign with the surname only.

Incorporated in Marat's letter is an account, by Bottineau himself, of the genesis of *Nauscopie*. For the sake of clearness, I print it separately.

"As early as the year 1762, holding then an inferior situation in the King's navy, it appeared to me that a vessel approaching land must produce a certain effect upon the atmosphere, and cause the approach to be discovered by a practised eye even before the vessel itself was visible. After making many observations I thought I could discover a particular appearance before the vessel came in sight: sometimes I was right, but more frequently wrong; so that at the time I gave up all thoughts of success.

"In 1764, I was appointed to a situation in the Isle de France: while there, having much leisure time, I again betook myself to my favourite observations.

"Here the advantages I possessed were much greater than before. First, the clear sky and pure atmosphere, at certain periods of the day, were favourable to my studies, and as fewer vessels came to the island, I was less liable to error than was the case off the coast of France, where vessels are continually passing, some of which may never arrive in sight, although the indications I allude to may have been witnessed by me. I had not been more than six months upon the island when I became confident that my discovery was certain, and that all that was requisite was to acquire more experience, and then *Nauscopie* would become a real science.

"As the officers in the island led an idle life, they were frequently on the shore looking through their glasses to discover when a vessel was arriving from Europe. I frequently laid wagers that a vessel was arriving, one, two, and even three days before she was actually in sight, and as I was very seldom wrong, I gained a considerable sum of money.

"The officers attributed my success to a particular

power of vision I possessed; but then again, they were quite puzzled on reflecting, that although they used glasses, I never employed any.

"In 1780, I wrote to the Minister of Marine, Maréchal de Castries, announcing my discovery. In his answer, he instructed the Governor of the island to enter my *announcements* of arrivals in a private register for two years at least.

"On the 15th May, 1782, my observations commenced.

"On the 16th May I announced to the Governor that three vessels were near the island. Orders were immediately given to the *vigies*;[1] their glasses were turned in the direction which I had pointed out. Their declaration was—'No vessel in sight.' On the 17th, the *vigies* informed that a ship had just appeared above the horizon. On the 18th, a second came in sight, and on the 26th, a third was visible to the naked eye.

"Viscount de Souillac sent for me on the last-named day, and made me an offer of 10,000 livres, and a pension of 1,200 livres a year, on the part of Government, if I would disclose my secret; but not conceiving the remuneration sufficient, I declined accepting the offer.

"Viscount de Souillac, after some months, wrote to M. de Castries: he stated, that I had made the surprising discovery of a new art—that of being able to observe the arrival of vessels 100, 150, and even 200 leagues distant; that for more than fifteen years I had regularly predicted the arrival of vessels, sometimes three or four days before they could be seen with a glass: that the register kept by order of the Minister showed that I had almost always been right in my predictions; and that even when I had announced the approach of a vessel which did not actually arrive, it was proved beyond a doubt that the vessel or vessels in question

[1] The official watchmen, or "look-outs".

were foreign ones that had come within two or three days' sail of the island, and had proceeded to their destination without touching at the Isle of France. 'Upon one occasion he asserted that a fleet of eleven vessels were approaching the island; the announcement caused great alarm, as we anticipated an attack from the English. A sloop of war was instantly dispatched to look out; but before she returned, M. Bottineau came to the Governor, and informed him that the signs in the atmosphere had disappeared, and that the fleet had taken a different direction. Some time after this a vessel arrived here from the East Indies, and reported that she had seen a fleet of eleven vessels sailing towards Fort St. William.' *In fine, that from the year* 1778 *till* 1782, *he had announced the arrival of* 575 *vessels, many of them four days before they became visible.*

"The letter terminated thus—'However incredible this discovery may appear, myself and a great many officers, naval and military, must bear testimony to the announcements made by M. Bottineau. We cannot treat him as an impostor, or as a visionary. We have had ocular demonstration for so many years, and *in no instance has any vessel reached the island, the approach of which he has not predicted*; those which did approach, but did not touch the island, were in most cases proved to be foreign vessels.

"A short time after this letter had been dispatched —(this letter, I am certain, reached the office of M. de Castries, but, I am also certain, was never perused by him)—I determined to return to my native country, and accordingly took my passage on board one of His Majesty's vessels, commanded by Captain Dufour. I felt somewhat anxious to ascertain whether the effect produced on the atmosphere, when a vessel approaches, would be somewhat similar, as regards the approach of one vessel towards another, and to my great delight I perceived it to be the same, although less powerful;

but my eyes now became so practised, that not once, during the voyage, did I make a mistake.

"I announced to Captain Dufour the approach of twenty-seven vessels, while proceeding to our destination; but (also) what afforded me more heartfelt satisfaction than my previous observations, namely, certain appearances in the skies when a vessel approaches land, the observer being on board; or similar appearances when one vessel approaches another; and, in my opinion, to be able to discover land from a vessel, long before it is in sight, is, if possible, of infinitely greater advantage to navigation.

"Upon one occasion, I told Captain Dufour that we were not more than thirty leagues from some land. This he denied to be possible: however, upon looking attentively at his reckoning, he was compelled to acknowledge that he was in error, and immediately altered his course. I discovered land three times during the voyage; once at the distance of 150 leagues.

"On the 13th June, 1784, I landed at L'Orient, and instantly proceeded to Paris. My applications to the Minister to obtain an audience were not attended to; and the only answer I obtained from the Officer of Marine was, that my memorial was under consideration. Abbé Fontenay, the editor of the *Mercure de France*, having heard of my 'pretended' discovery, without even asking to see my certificates, signed by the Governor of the Isle de France, and all the officers of the garrison there, thought proper to turn my discovery into ridicule, and affirmed that it was not 'ships at sea, but castles in the air' I had found out. In this state the affair remains; and all I can add is, that should vexation and disappointment terminate my existence before I can explain my discovery, the world will probably be deprived, for some time, of an art that would have done honour to the eighteenth century."

Poor Bottineau! Assuredly his discovery, could he have shown it to be explicable by the natural science of his time, would have made his reputation, and "done honour to the eighteenth century"; but in view of his proceedings at Mauritius the authorities to whose attention he brought it can scarcely be blamed for thinking that it smacked more of the Middle Ages. If he had wished to build up for himself a reputation as a practitioner of the Black Art, he could scarcely have set about it differently. His unceasing vigils, his definite predictions based (apparently) upon supernatural knowledge, their almost unvarying success, his assertion that he possessed a secret known to no other man, and his refusal to part with that secret even for what, to a man in his circumstances, represented affluence, might not impossibly cause mutterings and shaking of heads among his neighbours even in our own day; eighteenth-century Mauritius and eighteenth-century France probably numbered, even among their professed "free-thinkers", many whom not all the fulminations of Voltaire and the "appeals to reason" of the Encyclopædists could persuade out of a deep-rooted belief in sorcery.

As Bottineau predicted, the world was deprived of his discovery; as, no doubt, it has also been of many other discoveries made by poor and friendless men who have happened, or managed, to get on the blind side of officialdom. Bottineau was not the first man who has waited, covertly eyed by sneering messengers, in the anterooms of Government Departments: and who, as a rare alternative to the customary flat refusal of an interview, has trudged his way along the corridors, hoping against hope that his luck has turned at last—only to be received by some semi-detached private secretary, devoting such time as he can spare from his personal adornment to the neglect of his duties and the sometimes polite but invariably definite discouragement of "outsiders".

Yet there can be little doubt that he was no charlatan—

that he had made a discovery which would be of some interest even in these days of W/T, and must, in his own day, have been of much greater importance. Looking at the matter from the standpoint of the Vicomte de Souillac, it is a little difficult to understand how, in those days of "wars and rumours of wars", Bottineau ever managed to obtain from him permission to leave the island.

The French Governors of Mauritius, from Labourdonnais downwards, seem to have possessed, and exercised, a remarkable measure of absolute power. It is a matter of history that, some thirty years later, the unfortunate Captain Matthew Flinders, R.N., who, duly provided with a French safe-conduct, had done more for the survey of Australia than any one man before or since, was arrested at Mauritius in 1804 when on his way home to England, and was detained there till 1810 by the arbitrary fiat of the Governor, Decaen—and this in spite of the fact that the latter had received instruction for his liberation from Napoleon's Government as soon as the news of the arrest reached Europe. If De Souillac had compelled Bottineau to remain, or had demanded his secret as the price of letting him go, it is quite probable that the French Government, if they ever heard of this high-handed proceeding, would have endorsed it. And it might, in the long run, have been better had De Souillac acted thus. But whatever his motives may have been—and distrust in Bottineau's powers can scarcely have been one of them—he let him go his way to disappointment, neglect, and an unregarded death.

I turn to the certificates which the Abbé Fontenay so derided.

No. 1.

A letter from the Governor of the Isle of France to the Marshal de Castries:

"PORT LOUIS, ISLE DE FRANCE
"18.4.1784

"MONSEIGNEUR,—A letter which you wrote on the 6th of April, 1782, to M. Bottineau, a ci-devant officer of the second class in this colony, in the King's service, as well as in that of the company, renders it imperative on our part to give him one for you, of which he is the bearer.

"It is in order to be useful to his country that he is about to visit France; and he would experience much regret were his discovery lost to the world; a discovery with which he alone is acquainted, and which others have, in vain, attempted to unfathom; it consists of announcing the presence of one or several vessels at a distance of 100, 150, and even 200 leagues. Is this the the result of study, or the application of the principles of some science? By no means: all his science is in his eyes. He sees in nature signs that indicate the presence of vessels, as we assert that fire exists in places where we see the smoke; this is the comparison he makes, when speaking to others concerning his art; in keeping his discovery a secret, this is the clearest explanation he has afforded, in order to show that he did not make the discovery by the knowledge of any art, or of any science, or by the application of any previous study.

"He asserts, that it is the effect of chance that led him to the discovery; he has watched Nature, and found out her secret; this science, therefore, has not, it may be said, cost him any trouble: but that which has required much study, and really belongs to him, is the *art of judging distances*.

"The signs, he says, indicate clearly enough the presence of vessels, but *they only who can read the signs* are able to judge of the distances, and this art, he asserts, is an extremely laborious study. On this very account, he had for a long time been the dupe of his science; *for the last fifteen years he has foretold the appearance of vessels in this island.*

"At first it was merely a gamble: he was in the habit of making bets, and often lost them, because the vessels did not arrive at the appointed time: on this account, he studiously applied himself to find out the cause of his errors, and the perfection of his art is owing to his exertions.

"Since the war has broken out, his *announcements* have been very numerous, and sufficiently correct to create a sensation in the island. We have conversed with him upon the reality of his science; and to have dismissed him as a quack would have been an injustice. Moreover, we required proofs, and he regularly supplied us with 'announcements'; and the result was, that several vessels that had been announced several days beforehand, arrived at the precise time; several others were delayed, and several did not arrive.

"It has since been proved, that the delay in the arrival of some of the vessels was occasioned by contrary winds, or currents in the ocean. Those which did not arrive, M. Bottineau is fully persuaded, were foreign vessels which passed by: and, indeed, we have since ascertained that a fleet of English vessels arrived in India which might have been in sight of the island at the time fixed upon by M. Bottineau. *What we can certify is, that M. Bottineau was almost always right.*

"Whether this be the effect of chance, or otherwise, it would perhaps be imprudent in us to determine: this, however, is certain, that the circumstance is so extraordinary, in whichever way we consider it, that we endeavoured to prevail upon M. Bottineau to make us come to a positive conclusion, either by confiding his secret to us, or to any well-informed person who could be depended upon: but he declined to accede to our request; fearing, no doubt, that he should not obtain a sufficient remuneration for his discovery.

"We have the honour, etc.,

"LE VICOMTE DE SOUILLAC
"CHEVRAU"

No. 2

"The undersigned, chief officer of engineers of the King in the Isle de France, certifies, that M. Bottineau has, at different periods, announced to him the arrival of more than a hundred vessels, scarcely without ever being mistaken [*sic*]; that he announced these vessels, two, three, and even four days before the coast signals; and, moreover, that he stated when there was only one, or when there were several vessels.

"(*Signed*) GENU.
"TREBOND, *Colonel of Infantry*

"*November* 16, 1780"

No. 3

"I cannot refuse my testimony to truth, and I give this certificate in acknowledgment of the pleasure and agreeable surprise I have experienced from your continued and certain announcements. I advise you to cultivate this science, which will prove of immense benefit. The remarks of a few idle persons must not deter you. When Christopher Columbus proposed his discovery, he was treated as a madman by John the Second, King of Portugal, and Henry the Eighth, King of England; and had it not been for Isabella of Castille, who encouraged this celebrated Genoese, America would perhaps not yet have been discovered.

"This example, and a thousand others, show how prudent it is to withhold one's judgment on points of fact, in systems founded on astronomy or philosophy. I am persuaded that nature possesses a thousand secrets which are still hidden from us.

"(*Signed*) LE BRAS DE VILLEVIDERNE
"*H.M. Attorney-General for the Isle de France*

"5.11.1781"

No. 4

"We, Commissary-General of the Navy of this port, certify, that having wished to try whether M. Bottineau really possessed the talent of announcing (before the usual observers placed upon the mountains) the vessels that arrive here, and having desired him to inform us in writing of his predictions, he has announced to us within six months, one hundred and nine vessels, one, two, three, or four days before the signals were made from the mountains, and in this number he only was twice mistaken; moreover, he explained these errors by contrary winds or the currents. We have also to acknowledge, and not without great astonishment, that his art extends so far as to inform him whether there was one, or there were several in the vicinity of the Isle, and if they were together or separated.

"(*Signed*) MELIS

"PORT LOUIS, ISLE DE FRANCE
"16.5.1782"

Reading these certificates, it will be conceded that there is at least a *prima facie* case for conceding to Bottineau the power which he claimed of being able, by the recognition of some atmospheric phenomenon (which was, apparently, visible to any one whose eye had been trained to recognize it), to predict the approach of vessels of which every portion was still a long way below the horizon. No other explanation, I submit, is possible. Those who deny the authenticity of the documents which I have reprinted here are necessarily confronted by the fact that not only Brewster, but also many other people of his time have noted and expressed some garbled version of the story of "the wizard of Mauritius". It came, for example, to the front in the days when the mind of every intelligent person in our country was exercised over the problem of

finding traces of Sir John Franklin's ill-fated expedition.[1] In Brewster's day, there seems to be no doubt that the story of the ill-fated *Nauscopist* was a matter of "common fame", although, as often happens, the actual facts of the case were overlaid by a thick super-stratum of rumour.

I come now to the last of the documents which I shall reprint: Bottineau's own explanation of the feats which, I submit, there is no doubt that he accomplished. And there is an objection *in limine* of which I should like to dispose.

It has not infrequently been the case that the opinions of the intelligentsia and of οἱ πολλοί (which, as a matter of course, have differed) have ultimately been weighed in the balance of informed general judgment, and the popular opinion confirmed. On the literary side, I need only instance *The Pilgrim's Progress*, whose author Cowper dared not name. To-day, little as we may like Christian's haste to leave behind his wife and family in his eagerness to come first to the Golden Shore, we acknowledge (even without necessarily perusing all, or any, of the pages of that wonderful book) Bunyan's pre-eminence as an allegorist, and as the never-failing guide and solacer of those who are "fast bound in misery and iron". Similarly we know, as the result of the labours of E. F. F. Chladni (better known as the eponym of "Chladni's figures" and the founder of modern acoustics), that the ages-old theory of the origin of meteorites is, strangely enough, correct, and that the fine-spun theories of those scientists who stoutly denied, *a priori*, that they could possibly have an extra-terrestrial origin were, if possible, less related to the fact than the dogmas of any, or all, of the various churches which, being "no connection with the firm over the way", alone promise salvation.

If we seek for a modern case of an undisputed fact being

[1] See, for example, a letter (not otherwise remarkable) by one John Christopherson, published in the Parliamentary Papers relating to the Franklin search expedition under Capt. Austin, R.N. (1852).

discovered and remaining unexplained, we have not far to look. In 1922, Mr. E. A. Reeves, Map Curator of the Royal Geographical Society, published his discovery of the fact that a freely-suspended piece of paper, exposed to strong sunlight, will always tend to turn so as to set itself north and south. This fact has been repeatedly verified; but, so far, it has not been explained, either by its discoverer or by any one else. With this passing reference, I wish (greatly daring) to point out that it is entirely unfair to expect the discoverer of such facts to put forward their explanation; just as the Scotsman of Sir James Barrie's *Window in Thrums* was careful to explain that it was unfair to expect the maker of a joke necessarily to see the humour of it.

This, unfortunately, is a matter upon which, as upon all other subjects, public opinion is incorrectly informed. The discovery of some fact is one thing; the explanation of it quite another. Bacon saw this clearly, when he laid down the rules to be followed by his "Interpreters of Nature". Some men provide the facts: others interpret them. Occasionally, the two rôles are filled by the same man; but this is a rare event, not a natural consequence. The point is put very clearly by Wells (I have too high an opinion of his works to write "Mr. H. G. Wells", any more than I should write "Pierre Simon de Laplace", or "Charles John Huffham Dickens"[1]) in his story *Filmer*, which deals with a man who discovered an entirely safe method of flying, and killed himself rather than take part in an actual demonstration of his invention.

I remember coming across, many years ago, a book written by John Mullins, who acquired a competence and a considerable reputation as a successful "dowser", or diviner of subterranean water-springs. In his later years, he had been induced to put his experiences on paper: and, not content with recounting simply the facts of his

[1] Or, for that matter, "John Henry Brodribb Irving", or "Enoch Arnold Bennett".

own experience—the many cases in which he had indicated successfully the place of springs in situations far remote from any spots where ordinary geologists considered that there was a probability of finding water—he seems to have felt impelled to give his conclusions, and theory, as to the *modus operandi* which he employed. Though he did not know it, he was adding to an already considerable literature upon his own subject, and his contribution bore a strange and painful resemblance to a work which was published in 1546.

His book was reviewed, shortly after its appearance, in the scientific equivalent of *The Times*, that excellent paper *Nature* (of whose founder and first editor it was once said by Henry Smith, the Cambridge mathematician, that he exhibited "an arrogance which would still have been offensive even had he been the Author of Nature"). The review was entrusted to a deservedly-famous physicist who had done, and is still doing, work of first-class importance. With almost boyish glee, he fell upon his *corpus vile* much as, one imagines, Macaulay went to work upon the utterly inept productions of "Satan" Montgomery.

His review was scathing, clear, concise, damning—and absolutely unfair. In these days, "this ghastly, thin-faced time of ours", we hear, amid much waving of entirely bathetic red flags, frenzied and Klaxonic (or should one say Maxtonic?) appeals of "fair play for the underman". Well, if ever there was an entirely justified appeal of this kind, it was that of Mullins v. Authority. The subject-matter of the case—the question whether it is possible for certain specially gifted people to detect, by indications which are impalpable to most of us, the presence of underground water—may be regarded, without prejudice, as still *sub judice*. I will only say that those who have not read the late Sir William Barrett's book on the subject—and I have never yet met an opponent of the "dowser" who had—have still a little to learn; and that, for my own part, I consider that the power of detecting underground

water through one's personal sensations (of which the hazel twig, like the "planchette", is merely a convenient but by no means indispensable index) is one scarcely rarer than the ability to move one's ears independently—a faculty which I happen to possess myself. But whether this be so or not there can, I think, be little question that for a trained physicist to join issue with a well-meaning but imperfectly-educated man on the subject of phenomena which the latter honestly professed to have observed, *not on the facts but upon the explanation of them*, was to gain a Pyrrhic victory by the sacrifice of all traditions of fairness, sportsmanship, and scientific candour. The motto of a celebrated society to which the reviewer belongs is *Nullius in Verba*, and its leading principle has always been that of the man from Missouri—"It's up to you to show me." I could give many instances from its history in which one man has brought the facts of a certain phenomenon into notice, and another has explained them. To gird at a discoverer because he has not hit upon the true explanation of his observations is not far removed from the sin of Ham.

With these petulant but not entirely irrelevant remarks, I return to Bottineau. Here are some notes which have survived upon the subject of his discovery, as explained by himself. It is exasperating to reflect that his secretive "complex", to use a word beloved of those who minister to the ailments of half-witted old women of both sexes, has prevented him, even here, from giving a clear description of the "signs" upon which his "art" was based.

> "NAUSCOPIE is the art of ascertaining the approach of vessels, or, being in a vessel, the approach to land, at a very great distance. The knowledge neither results from the undulation of the waves, nor from quick sight, nor from any particular sensation, but simply from observing the horizon, which bears upon it certain signs indicative of the approach of vessels, or land.

"When a vessel approaches land, or another vessel, a *meteor*[1] appears in the atmosphere, of a particular nature, *visible to every eye*, without any difficult effort: it is not by the effect of a fortuitous occurrence that this meteor makes its appearance in such circumstances; it is, on the contrary, the necessary result of the approach of one vessel towards another, or towards land. The existence of this meteor, and the knowledge of its different modifications, constitute the certainty and the precision of my announcements.

"If I am asked how it is possible that the approach of a vessel towards land can cause any meteor to be engendered in the atmosphere, and what affinity exists between two effects so removed; I reply, that I must be excused giving an account of the *why* and the *wherefore*; that it is sufficient I have discovered the *fact*, without being obliged to explain the principle.

"Do not even the learned acknowledge that the explanation of meteors is often beyond their comprehension? Valmont de Bomarre says, 'Almost all meteors present in the mechanism of their formation considerable difficulties, profound mysteries, which all the knowledge of philosophers has not yet been able to penetrate.'

"After this avowal, it certainly is not my province to explain what the most learned men declare to be inexplicable.

"The meteor of which I am speaking, although manifesting its effects, may conceal its principle; and, notwithstanding, my discovery does not the less exist.

"However, the study of twenty years seems to have given me a right to reason upon a subject that has become so familiar to me; and the following is my opinion on this head.

"The vast expanse of water forms an immense abyss,

[1] i.e. an atmospheric effect (Fr. "météore"—cf. our "meteorological"). Compare Gray's *Bard*:

". . . his hair
Streamed like a meteor on the troubled air."

into which substances of all kinds are continually entering. The enormous number of animals, fishes, birds, vegetable and mineral productions, which are decomposed in the vast body of water, produce a continual fermentation of matter, which abounds in spirit of salt, sulphur, bitumen, etc. The presence of these gases is sufficiently apparent, from the smell and disagreeable taste of sea-water. These gases, closely united with the sea-water, remain stationary so long as the waters are quiet, and not disturbed; or they may only experience a slight internal agitation, which is manifested externally in a small degree.

"But when the water is put in motion by stormy weather, or by an active mass which passes over its surface with violence and rapidity (a vessel, for instance), then the volatile vapours that are enclosed within the bosom of the deep escape, and rise in smoke (*fumée*) composing a vast envelope around the vessel. As she advances, the envelope advances with her, increased every moment by fresh emanations. These emanations are so many particular clouds, which, by degrees joining each other, form a kind of cloud (*nappe*) that projects forward, one extremity of which touches the vessel, while the other extremity advances to a considerable distance. This train of vapours is not on that account visible; it escapes observation by the transparency of its parts. And it is lost among the other fluids that compose the atmosphere: but as soon as the vessel reaches a situation in which it meets with other homogeneous vapours, such as those which escape from the earth, one perceives, on a sudden, that cloud (*nappe*) until then so limpid and subtle, acquire consistence and colour, by the mixing of the two opposed columns. The change commences at the prolonged extremities, which unite by contact, and are thus strengthened and coloured; and then, every minute, as the vessel advances, the change is graduated, gains the centre, and at length,

the *engrainement* being complete, the phenomenon becomes more manifest, and the vessel appears.[1]

"Such, in a few words, is the revelation of the cause and the effects of a phenomenon which, however wonderful it may be, accords, notwithstanding, with physical notions.

"Whatever cause may be assigned for this phenomenon, it is quite certain that it is the infallible satellite of a vessel; and that, in consequence of its prolonged form, it manifests itself to the eyes one, two, three, four, five, and even six days before the vessel itself, according to the state of the weather and the nature of the obstacles it meets with. When the vessel sails with a fair wind (*en poupe*) and meets with no obstacle, the phenomenon possesses its greatest celerity; and, arriving several days before the vessel, it affords the observer the means of announcing the presence of a vessel at a considerable distance; but when the vessel meets with contrary winds, it will be understood that this circumstance must have a great influence on the progress of the phenomenon. On this account, I state that the phenomenon sometimes appears four or five days before the vessel, and sometimes only one day. This defect of uniformity in the apparition results from the greater or less impediment it meets with.

"It will naturally be supposed that there may be weather when the phenomenon cannot show itself before the vessel: for instance, in a violent gale, which appears, at first sight, capable of carrying away the phenomenon—even dissipating and entirely destroying it. This, however, is not the case. The most impetuous wind only retards the apparition of the phenomenon, without destroying it. But when the vessel has reached a certain distance from land, then the phenomenon has acquired so much consistence, that it

[1] One could wish that the translator, when dealing with this portion, had consulted some one who could read French without a dictionary.

overcomes the efforts of the strongest winds, which, though they agitate it, still leave some part which they cannot wholly disperse.

"The whole of my science consists in being able to follow the apparition of this meteor, and distinguish its character, in order not to confound it with the other clouds in the atmosphere, and which are not to be attended to. In order to make these observations, neither telescopes nor mathematical instruments are required; the eyes alone are sufficient.

"It is not even necessary to be upon the coast; where the horizon of the sea can be discovered, the observer can announce the approach of vessels.

"The cloudy mass does not present itself suddenly, and with all its character. The first appearance is equivocal, and only puts the observer upon his guard, who can then commence his study, without being in haste to certify that the vessel is arriving; but, by degrees, the forms are developed, the colours assume a certain tone, the volume acquires consistence, so that the *Nauscopist* can no longer doubt that a vessel is behind; because these forms and these developments are such, that they can only belong to these kind of vapours.

"As the vessel advances, the meteor extends, and becomes consistent. From the moment I became familiar with this singular analogy, I never failed seeing my announcements followed by complete success; and this punctuality caused the great astonishment, mentioned in my certificates, etc., from the Governor, officers, and inhabitants. Convinced of the effect, but not understanding the cause, they could not conceive that a science existed which could give to Man a foreknowledge of events so distant, with respect to time and place. The people attributed these events to the power of magic; the better informed ascribed them to chance. Nothing, however, is more natural than this

principle, which has astonished every one, and concerning which so much incredulity will be manifested throughout Europe.

"BOTTINEAU"

That is the last of the "original documents"—if one can apply that term to rough translations, by an unknown translator, from originals which have long since disappeared, and whose provenance is entirely uncertain. Yet, as if to provide one little glimmer of light, the translator has kindly added a note, which is given below.

"The *Gazette de France* mentions this discovery; and the Abbé des Fontains wrote several articles upon the subject; but the public mind was at that period so absorbed in matters of political importance, that the unfortunate M. Bottineau was neglected; and a letter from Marat, at the dawn of the French Revolution, merely states that Bottineau had died. The different biographical dictionaries we have consulted make no mention of him."

The editorial "we" in the last sentence suggests that Becher himself was the translator—which is not at all unlikely. His searching of "the various biographical dictionaries" is evidence of his thoroughness, if not of his common sense. It was scarcely likely that Bottineau would secure the smallest niche in such tomes; he was more likely to have found his way into such a work as Brewer's *Dictionary of Phrase and Fable*. But unfortunately he did not do so. My search in all likely and unlikely quarters has had no better result than Becher's; and we are driven to fall back upon such information as can be extracted from the documents already quoted.

I have already indicated my opinion that the story of the wizard of Mauritius is substantially true. This does not mean that I suggest we should accept every part of it

au pied de la lettre. Some hardened sceptics may deny the whole story; they may suggest that Bottineau never existed, and that Becher concocted the whole thing out of his own head. This theory is attractive by reason of its simplicity; but it breaks down in face of the incontestable fact that Brewster, who speaks of the matter as a thing of common knowledge, wrote the last of his Letters in 1832, while the account of Bottineau in the *Nautical Magazine* did not appear until March 1834.

If, however, we accept the fact that a man named Bottineau once resided at Mauritius and, justly or unjustly, acquired a reputation for being able to predict the advent of vessels which, like the Spanish Armada referred to in the *Critic*, could not be seen because they were not yet in sight (and this starting-point is one which, I suggest, very few will refuse to occupy), it becomes a matter for one's individual judgment how much further one can reasonably go. In what follows, I only give my own views, for what they may be worth.

If we give any credence to the certificates, the theory that Bottineau's successes in predicting the arrival of ships at Mauritius were purely a matter of chance must, I think, be dismissed as untenable. In view of the chances against him, to suppose that his exploits were a matter of guesswork involves greater improbabilities than it removes. Let us assume, then, that he had acquired some peculiar skill in observing, and making deductions from, indications which were not apparent to the average man.

Now what were those indications? His language, even allowing for the inevitable crudities involved in "doing into English" what was, probably, not the most classical French, is very obscure. But it seems possible to extract from it both positive and negative evidence.

Neglecting his own theory, except in so far as it gives some indications of the nature of the appearances which he saw, or thought that he saw, the reasonable deductions seem to be these. He had trained himself to see some very

faint appearance, like an extremely attenuated cloud, somewhere near the sea-horizon, and he found, by experience, that this heralded the advent of a ship. He speaks of the "form" and "colour" of such clouds, and of their acquiring "consistency"; but there can be little doubt that even when most fully developed this appearance must have been far from easy to see. There must have been many people in Mauritius who, stimulated both by his example and by the hope of gaining some such reward as was offered to him by De Souillac, strained their eyes, during the interval between one of his announcements and its fulfilment, to discover the data upon which that announcement had been made. Their non-success proves, if it proves nothing else, that "the phenomenon" was not easily perceptible. Bottineau's own statements support this; he remarks that a person practising his "science" must undergo a short period of education; sure proof that his "meteor" did not leap to the eye.

It will, I suppose, always be a matter of doubt how far he may have been assisted by unusually keen vision. Such he may have possessed—and, indeed, probably did possess; but the importance of such a faculty is very easy to overestimate. In astronomy, for example, it has often been shown that observers using telescopes of quite moderate power have been able to confirm and reobserve discoveries originally made with the great telescopes—*once they knew where to look for them.* The satellites of Mars, originally discovered by Asaph Hall in 1877 with the 26-inch Washington telescope, and often reobserved with a 12-inch aperture, are a good example of this. It seems likely that Bottineau, scanning the horizon, saw what others failed to see simply because he had trained his eye to detect faint indications which were invisible to an ordinary observer who did not know what they looked like.

It will always be a matter for regret that (so far as I am aware) Bottineau's narrative never came into the hands

of Charles Meldrum (1821–1901), the great meteorologist to whom we owe the first scientific investigation of the laws governing the cyclones of the Indian Ocean, and who was in charge of a meteorological observatory at Mauritius itself from 1862 until 1896. I do not think, from what I know of his character, that he would have scoffed at it; rather do I imagaine that he would have devoted considerable time and trouble to making a full investigation of the subject, keeping an open mind until he had accumulated sufficient data to form a considered opinion.

However, the matter is not past mending. If there is any truth in Bottineau's statements as to the general character of the phenomena which he was able to detect, any observer with a camera and a clear sea-horizon, whether at Mauritius or elsewhere, who takes a series of photographs of the sea-horizon at short intervals spread over a considerable period of time, and checks them against a register (obtained by W/T or otherwise) of the movements of shipping within a 200-mile radius, should be soon able, if Bottineau's "meteor" has any real existence, to announce to the world an important rediscovery; while, if he draws blank, the "wizard of Mauritius" will remain, more than ever, an enigmatic figure—far beyond praise or blame—generally regarded as a charlatan, but actually a member of that unfortunate fraternity to whom the world has never listened, because they have not prophesied acceptable things.

Postscript

In 1817 and 1818 Capt. the Hon. Francis Maude, R.N. (1798–1886) then a midshipman in H.M.S. *Magicienne*, frequently saw at Mauritius an old resident who had learned the art of "Nauscopie" from Bottineau himself, and habitually practised it—with unvarying success. I owe this information to a small pamphlet, *Notes of a Life* (London, 1883) kindly given to me by Capt. Maude's son.

IV

THERE WERE GIANTS IN THOSE DAYS

In the year 1577 a skeleton of enormous size was discovered at Willisau, in the canton of Lucerne, Switzerland. The local authorities, undecided as to the nature of the remains, which were incomplete, and doubtful whether (as in the famous case of the Peruvian mummy left unclaimed in the parcels-office of a London station[1]) they ought not to hold an inquest and/or give them Christian burial, consulted the famous Dr. Felix Plater, of Basle, the most expert anatomist (strange though it may seem) of his day. Plater gave it as his opinion that the bones were undoubtedly human, and forwarded to Lucerne an anatomical drawing of their original owner, who must (according to this) have stood some nineteen feet high.[2] The "Giant of Lucerne" lay in state at the town hall of Lucerne until, in an evil hour, he was visited by a still more competent anatomist than Plater—one J. F. Blumenbach (1752–1840), of Göttingen, a savant possessed in full or even brimming measure of that irritating turn of mind which takes nothing for granted. One glance at the bones was enough for him—but it was no easy matter to convince the good citizens of Lucerne, who had adopted the Giant as one of the supporters of their city arms, that he was only a mammoth.

Giants of this kind had a habit of turning up, during the Middle Ages and even later, in all parts of Europe. For example, in 1613 the learned world was astounded to

[1] This inquest was actually held, and the very reasonable verdict returned that the deceased had come by his death so long ago that the cause of it could not be ascertained. As the law then stood (it has since been altered), the holding of an inquest, even in such ridiculous circumstances, was imperative.

[2] See Fig. 10.

hear of a giant found by some workmen in a sandpit near the castle of Chaumont, not far from St. Antoine. At a depth of 18 feet they discovered a brick tomb 30 feet long, 12 feet wide, and 8 feet high. In this lay an entire human skeleton, over 25 feet in length, 10 feet across the shoulders,[1] and 5 feet deep from the breast to the back.

10. The Giant of Lucerne. Re-drawn from an engraving in J. L. Cysat's *Beschreibung dess berühmbten Lucerner* . . . (Lucerne, 1661).

Furthermore, an inscription in Gothic characters on the lid of the tomb read "Teutobochtus Rex", and demonstrated, in conjunction with medals, coins, etc., found near by, that the body was that of Teutobochtus, the giant king of the Cimbri, who was defeated and captured by Marius, near Aix, in Provence; and whose head, as he walked behind his conquerer's triumphal car, is recorded by Florus[2] to have overtopped the trophies carried on the Roman spears (as well it might). The details of this truly

[1] This, in itself, was suspicious. The breadth of the shoulders in man is generally not much more than a quarter of the height.
[2] In his *Epitome de Gestis Romanorum.*

amazing find were vouched for by one Dr. Mazurier,[1] a surgeon, who shortly afterwards issued a pamphlet about them—the first shot of a long battle.

The genuineness of Mazurier's account was violently attacked; notably by one Jean Riolan, a celebrated naturalist of the period, in two tracts with the snappy little titles of *Gigantomachia* and *Gigantologie*. In all, over a dozen pamphlets were hurled to and fro. Finally, it was made clear that Mazurier had bought from the workmen various enormous bones which they had found in the pit, and had supplied the rest of the details himself. Teutobochtus, restored to his original configuration—that of a mastodon—is now in the Musée de Paléontologie, Paris.

In the time of James I, certain "big, outlandish bones" were discovered at Gloucester, and Lord Herbert of Cherbury was appointed to find out what he could about them. He was assisted (or, perhaps, impeded) by several pundits, some of whom advised the re-interment of the bones with Christian rites; while others, including the great William Harvey, declared them to belong to "some exceeding great beast, as an elephant". One of them, Bishop Hakewell, remarks:

> "His Lordship showed me some bones, which he had collected; which were a huckle-bone,[2] part of the shoulder-blade, some parts of a tooth, and the bridge of a nose—all of a huge bigness. . . . The bridge of the nose was what confirmed his lordship's and my opinion, that it could not be that of a man, for it did seem to be a bone very apt to bear up the long snout of an elephant. One of the teeth of this pretended giant, by the special favour of my lord of Gloucester, was examined by me. I found it to be a stony substance,

[1] He seems to have used the pseudonym "Nicholas Habicot". Can Voltaire have confused him with his " . . . *coquin d'Habacuc, capable de tout*"?

[2] i.e. hip-bone.

both for hardness and weight; and it should seem, by his lordship's letter to me, that he himself was not confident that it was the tooth of a man."

As Greenwood[1] has pointed out, in those days simple arithmetic seems to have been as unknown a science as comparative anatomy. The tooth in question, if that of an elephant (which, no doubt, it was) would have weighed some ten pounds. Human teeth scale about 160 to the pound; and in the same proportion the owner of the tooth, if he were human, would have weighed about a hundred tons.

But, while such blunders seem almost incredible to-day, there are two potent factors to be borne in mind. Until the very end of the eighteenth century it was generally believed that the fauna of a given locality must have persisted, unchanged, since the Creation; and when, for example, Cuvier first announced the discovery of elephant, hippopotamus, and rhinoceros remains in the upper European strata, he was gravely informed that these must belong to the elephants brought from India by Pyrrhus.

And, secondly, a general belief in the existence of various giant races of men seems to be almost as old as humanity. The mythologies of all races and all creeds are full of giants—their existence is one of the most widely spread of all beliefs. To our forefathers, it was an immeasurably more natural thing to discover the bones of a giant than those of an elephant. Sometimes, indeed, one comes across a reputed giant who could scarcely have been an elephant—witness the following:

"A True Report of Hugh Hodson, of Thorneway, in Cumberland, to S^r. Ribert Cewell, of a gyant found at S. Bees, in Cumb'land.

"The said gyant was buried 4 yards deep in the

[1] *Wild Sports of the World*, J. Greenwood, London, 1862.

ground, w^ch is now a corn feild [*sic*]. He was 4 yards and a half long, and was in complete armour: his sword and battle-axe lying by him. His sword was two spans broad, and more than 2 yards long. The head of his battle-axe a yard long, and the shaft of it all of iron, as thick as a man's thigh, and more than 2 yards long.

"His teeth were 6 inches long, and 2 inches broad; his forehead was more than 2 spans and a half broad. His chine bone could contain 3 pecks of oatmeale. His armour, sword, and battle-axe are at Mr. Sand's, of Redington, and at Mr. Wyber's, at St. Bees."[1]

It would have simplified matters if only the bones, or the armour, had been found. The bones would naturally have been those of an elephant, while it was not unknown for chiefs in bygone days to have some of their personal property made of exaggerated size, so as to give a false idea of their strength and stature. As this "True Report" stands, one can only conclude that Hugh Hodson, of Thorneway, in Cumberland, had the makings of a very expert witness.

The Patagonian Giants

But the medieval believers in a giant race were not satisfied by merely finding relics of it, however large. They argued that such a race must still exist; and it so happened that while Magellan was showing the way round the world he incidentally provided the credulous with a legend—that of the gigantic natives of Patagonia—which exercised men's minds fully for three centuries. These may be divided, roughly, into a century of credulity, a century of incredulity, and a century during which it became apparent that the story of the Patagonian giants

[1] Quoted from Jefferson's *History and Antiquities of Allerdale above Derwent*, and stated to be taken from the Machel MSS. (Carlisle), vol. vi.

had a real, if slender, basis of truth. Here is a short outline of the events of these three periods.

The First Period

The discovery of Patagonia[1] is due to Magellan, who coasted its eastern shores, and passed through Magellan Strait, which divides it from Tierra del Fuego, in 1520.[2]

While Magellan's fleet was lying in Port San Julian[3] in June 1520, a gigantic native appeared on the beach near the ships. "This man", says Pigafetta, a companion of Magellan, "was so tall that our heads scarcely came up to his waist, and his voice was like that of a bull." He was well treated, and other natives soon appeared, of whom Herrera,[4] less hyperbolic than Pigafetta, remarks only that the smallest of them was taller and bulkier than any of the Spaniards.[5] Magellan gave them the name "Patagones" ("Pata" being Spanish for "hoof"), from the guanaco-skin moccasins which they wore. By a stratagem he managed to put two of them in irons,[6] intending to take them to Europe. Both died before crossing the Line. Pigafetta managed to compile a short glossary of their principal words, in which it is interesting to find the name "Setebos" used (as, later, in *The Tempest*) for their principal deity.

[1] Patagonia, as the geographical name of a region, covers all land between the Rio Negro (in approximately 39° S.) and the Magellan Strait; and hence includes the southern portions of Argentina and Chile. The region supposed to be inhabited by giants was in the near vicinity of the Strait.

[2] Here, and later, I have generally used the epitomes of the early voyages compiled by such writers as Hawkesworth and Burney.

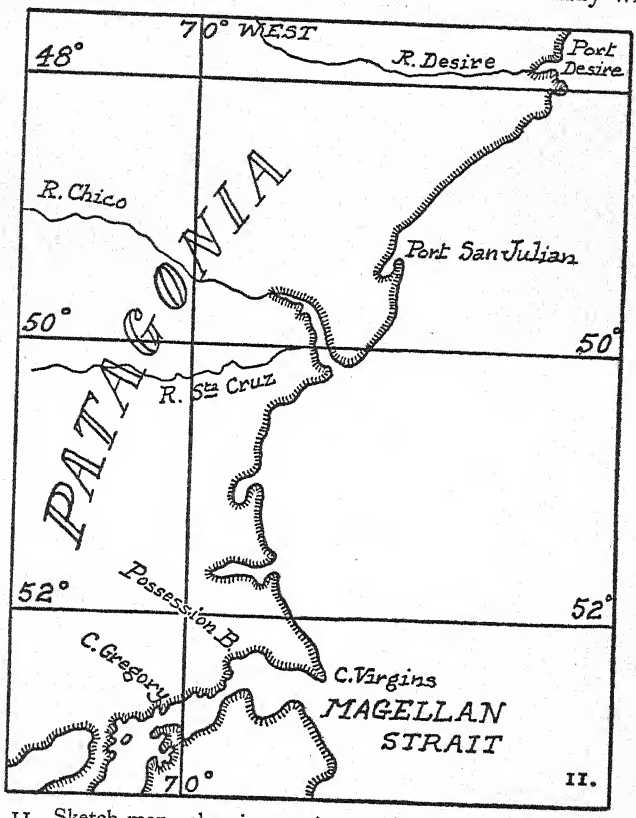
[3] 49¼° S., 68° W.—on the Argentine coast. It was here that Magellan strangled Quesada, one of his mutinous captains; and here also that Drake beheaded Thomas Doughty. See Fig. 11.

[4] Official historian of the Spanish voyages to the New World. He did not take part in any.

[5] According to Max. Transylvanus, the tallest Patagonian seen stood 7 feet 6 inches.

[6] Two others were, with difficulty, overpowered by nine of the Spaniards and bound; one, however, broke loose, and the other got away later.

Drake, who anchored in Port San Julian in June 1578, also saw "men of large stature", and had an affray with



II. Sketch-map, showing various localities in which the "Patagonian Giants" have been encountered.

them, losing two of his crew. They are described as standing a little under 7 feet 6 inches.

Pedro Sarmiento, in February 1580, saw "people of large stature" in Magellan Strait, but their height is not stated; while Tomé Hernandez, who had spent about a

year in the Strait in 1584, could say nothing about the stature of the natives, except that they were "very corpulent". As he nearly died of starvation there, he probably felt strongly on the subject.

Anthony Knyvet, who accompanied Cavendish in his disastrous second voyage and traversed the Strait in April 1592, speaks[1] of having seen Patagonians 14 to 16 spans (i.e. 10½ to 12 feet) in height; and also of having measured several dead bodies of the same size at Port Desire.

Sebald de Weert, who visited the Strait in 1598, speaks of the natives as being 10 or 11 feet high; and Joris Spilbergen, on April 3, 1615, saw "a man of extraordinary tall stature" watching his ships from the south side of the eastern entrance. On an island near by his men found the dead bodies of two natives, half-buried. One was of ordinary stature, but the other 2½ feet taller.

Jacob Le Maire and Wilhelm Schouten are said, but on doubtful authority, to have found skeletons 10 or 11 feet long at Port Desire in December 1615. Aris Clatz, Le Maire's "Commissary" (supercargo), is supposed to have made the discovery, but it is not mentioned in the earliest account of the voyage.[2]

The Second Period

It might be thought that, by this time, the existence of men much above the common height in Patagonia was fairly well established. But such was not the case. The voyagers of the late sixteenth century, as a class, seem to have gone out of their way to point out that they had seen no gigantic Patagonians (which was no doubt true); and, as a natural deduction, that none existed (which was unwarrantable). For example, Sir John Narborough,

[1] In his *Relation* . . ., for which see Purchas. Burney speaks of it as ". . . a Relation by Anthony Knyvet, which contains many things not credible".

[2] *Journal of the Voyage of Wilhelm Schouten* (Amsterdam, 1617). Clatz's story appeared in the second (1619) edition.

who spent ten months on the Patagonian coast in 1670, expressly denied that the Patagonians were any taller or bigger than other men. He attested that he had often measured the skulls and footprints of the savages whom he met in Magellan Strait, and found these to be of ordinary size; and that the same was the case with the numerous natives whom he had seen at Port St. Julian. It was recalled that John Winter, Drake's disaffected second-in-command, had stated on his return to England that he had seen no giants in the Strait, and that their existence was a fable invented by the Spaniards; and de Gennes, who followed Narborough in 1696, supported his views. On the other hand, Captains Harrington and Carman, commanding two French ships,[1] saw giants repeatedly in Possession Bay; six on one occasion, seven on another, and yet again "about four hundred men, part of whom were gigantic and part of the common stature". And Frezier was informed by the Spanish authorities at Valdivia, Chile, in 1712, that a tribe of gigantic natives, averaging 9 to 10 feet in height, existed in the interior of southern Patagonia. Oliver van Noort, in 1599, had heard the same story from natives (of the ordinary size) whom he took on board in Magellan Strait and, somewhat barbarously, instructed in the Dutch tongue.

The Third Period

The visit of Commodore Byron to Magellan Strait in 1764 was long regarded as having definitely established the existence of the Patagonian giants. Even before his time, however, it had come to be recognized that Patagonia was a large place; that its interior was unknown; that it was apparently inhabited by various nomadic tribes differing widely in many ways; and that no inference drawn from the stature of the natives encountered at a particular place could be relied upon to indicate what

[1] One appears to have been the *Jaques* of St. Malo.

would be met with elsewhere by the same voyager, or at that place by a later one.

Byron, in the *Dolphin*, anchored inside C. Virgins, at the eastern entrance to the Strait, on December 21, 1764. As he anchored he caught sight of a number of natives on horseback,[1] waving to invite the strangers on shore. Accordingly, he landed with an armed party, and was at once confronted by a native who appeared to be a chief. The remainder of the Patagonians, some five hundred, kept at a little distance. Here is Byron's (or, rather, Hawkesworth's)[2] account of the meeting:

> "One of them, who afterwards appeared to be a Chief, came towards me: he was of a gigantic stature, and seemed to realize the tales of monsters in a human shape: he had the skin of some wild beast thrown over his shoulders. . . . I did not measure him, but if I may judge of his height by the proportion of his stature to my own, it could not be much less than seven feet. When this frightful Colossus came up, we muttered somewhat to each other as a salutation, and I then walked with him towards his companions. . . ."

Later, when friendly relations had been established, he is made to remark:

> "Mr. Cumming came up with the tobacco, and I could not but smile at the astonishment which I saw expressed in his countenance, upon perceiving himself,

[1] Bulkeley, a survivor from the wreck of the *Wager*, speaks of having seen three mounted Patagonians near C. Virgins on December 12, 1741. This is the earliest recorded instance of their having been seen to use horses.

[2] Dr. John Hawkesworth (*ob.* 1773) was paid £6,000 by the Admiralty for editing the journals of Byron, Wallis, Carteret, and Cook (first voyage), which he published in three ponderous volumes just before his death. He was a disciple of Johnson, and in consequence emended the plain English of Byron and the others into Johnsonese, often with absurd results.

though six feet two inches high, become at once a pigmy among giants; for these people may indeed more properly be called giants than tall men; of the few among us who are full six feet high, scarcely any are broad and muscular in proportion to their stature, but look rather like men of the common bulk, run up accidentally to an unusual height; and a man who should measure only six feet two inches, and equally exceed a stout well-set man of the common stature in breadth and muscle, would strike us rather as being of a gigantic race, than as an individual accidentally anomalous; our sensation, therefore, upon seeing five hundred people, the shortest of whom were at least four inches taller, and bulky in proportion, may be easily imagined."

The *Annual Register* for 1768 contains an account of the same meeting, written by Mid. C. Clerke,[1] one of Byron's officers. He says:

". . . some of them are certainly nine feet, if they do not exceed it. The commodore, who is very near six feet, could but just reach the top of one of their heads, which he attempted, on tip-toe; and there were several taller than him on whom the experiment was tried. They are prodigious stout, and as well and proportionably made as ever I saw people in my life. . . . The women, I think, bear much the same proportion to the men as our Europeans do; there was hardly a man there less than eight feet, most of them considerably more; the women, I believe, run from 7½ to 8."

These remarks show the writer to have been misled by that curious illusion which makes most people overestimate the height of any one who is considerably taller than they are. By experiment, it will be found that the man whose head Commodore Byron could barely touch

[1] Afterwards Cook's second-in-command, 1776.

must have stood just about 8 feet—or, if anything, an inch or so less.[1]

Wallis and Carteret, who visited C. Virgins two years later (December 1766), took more accurate measurements of the natives (not necessarily, of course, the tribe seen by Byron). Wallis states:

> "As I had two measuring rods with me, we went round and measured those that appeared to be tallest among them. One of these was six feet seven inches high, several more were six feet five and six feet six inches; but the stature of the greater part of them was from five feet ten to six feet."

These measurements were in agreement with some obtained a little earlier (May 1766) by Duclos Guyot, one of Bougainville's captains, at a point in the Strait a little westward of C. Virgins. One of his officers measured the shortest of six natives whom he met, and found his height to be a quarter of an inch under 6 feet. He contented himself with noting that "the others were considerably taller".

Hawkesworth, summing up all the evidence then (1773) available, reached the conclusion that the more settled natives on both sides of the Strait were of ordinary stature, and that the "giants" spent most of their time on the western side of the Andes, and elsewhere in the interior —only visiting the shores of the Strait at rare intervals. He winds up his analysis by remarking triumphantly:

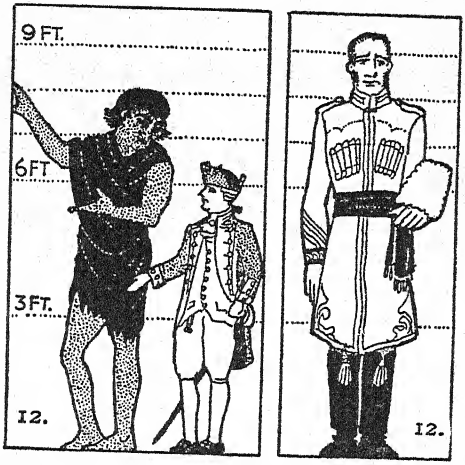
> "Upon the whole, it may be reasonably presumed, that the concurrent testimony of late navigators, particularly Commodore Byron, Captain Wallis, and Captain Carteret,[2] Gentlemen of unquestionable veracity, who

[1] I stand 6 feet 4½ inches, and span 6 feet 8 inches from finger-tip to finger-tip: and I find that (standing on tip-toe) I can just touch a point, on a vertical wall, 8 feet 6·3 inches from the ground.

[2] It will be noticed that he makes no mention of Cook. The latter, as we might expect, wasted no time in the Strait, but went round the Horn.

are still living, and who not only saw and conversed with these people, but measured them, will put an end to all the doubts that have hitherto been entertained of their existence."

But if these "prave 'ords" gladdened the hearts of Lord Monboddo[1] and other cranks, they were not confirmed



12. *Left*, Commodore Byron and a Patagonian—from a plate in Hawkesworth's *Voyages*. *Right*, Fedor Machnov.

by later explorers. No one has yet obtained definite proof that Hawkesworth's supposed giant race actually exists. On the other hand, there is plenty of testimony to the great average stature of many Patagonians. Darwin[2] says:

[1] James Burnet, Lord Monboddo (1714–99), was a Scottish lawyer, chiefly remembered by the pertinacity with which he defended his remarkable theory that all children are born with tails—these, in civilized countries, being surreptitiously removed by the midwives. He was also a strenuous advocate for the existence of the Patagonian giants. In justice to the memory of a much-derided man, it should be pointed out that he was far in advance of his time in maintaining that a close relation existed between the physical structure of man and of the higher apes.

[2] *Voyage of the "Beagle,"* chap. xi.

"During our previous visit (in January),[1] we had an interview at Cape Gregory with the famous so-called gigantic Patagonians, who gave us a cordial reception. Their height appears greater than it really is, from their large guanaco mantles, their long flowing hair, and general figure: on an average their height is about six feet, with some men taller and only a few shorter; and the women are also tall; altogether they are certainly the tallest race which we anywhere saw."

And Bourne, who spent some time among them *circa* 1849, remarks:

"In person they are large; on first sight they appear absolutely gigantic. They are taller than any other race I have seen, though it is impossibe to give any accurate description. The only standard of measurement I had was my own height, which is about five feet ten inches. I could stand very easily under the arms of many of them, and all the men were at least a head taller than myself; their average height I should think is nearly six and a half feet, and there were specimens that could have been little less than seven feet high."

Believers in a living race of giants, if they are not satisfied with the Patagonian's average height of 6 feet or a little over (which is considerably above that of any other race) may, if they wish, still believe that the 9-foot men alleged to have been seen by Byron (or, for that matter Knyvet's men of 10 to 12 feet) were stray members of a tribe of colossi still surviving in the heart of Patagonia. It cannot definitely be said that such is an impossibility. There are enormous areas in southern Patagonia which are still quite unexplored. It may be remembered that a considerable sensation was caused in 1897–8 by the discovery, in a cave at Consuelo Cove, Last Hope Inlet, on the western coast of Patagonia, of what was, apparently,

[1] 1834.

some quite fresh skin from a Mylodon, or giant sloth—an animal hitherto supposed to have been extinct since prehistoric times. An expedition,[1] sent out to test the supposition that surviving specimens of the Mylodon might still be found in some remote and unknown regions of Patagonia, left the question undecided; but it showed conclusively that very much more exploration would have to be effected before the theory could be rejected as impossible.

If we do not wish to locate our giant race in South America, we can fall back on the recent reports of the mysterious "Migues" of the Himalayan slopes; who are stated (by their native neighbours) to be from 10 to 12 feet high.[2] They have never been seen by a white man, although the Mount Everest expeditions heard some vague rumours about them.

But we need not go far to find an actual race of giants—although they are an artificial product. They are to be found where one would scarcely think of looking for them—in Japan. The average Japanese wrestler is of huge size, sometimes exceeding 7 feet, and of more than corresponding bulk. There is a delightful description of them, and of the incredibly funny ceremonies attending their contests, in the late Lord Curzon's *Tales of Travel*—a book which incidentally reveals its author as possessed of a very keen sense of humour. He compares these vast beings, very justly, to Daniel Lambert[3]—and speaks of their evolution thus:

"I afterwards inquired how it was that this strange

[1] Led by the late H. Hesketh Prichard, and financed by the late Sir C. A. Pearson. It did not succeed in reaching Last Hope Inlet. See *Through the Heart of Patagonia*, by H. Hesketh Prichard (London, 1902).

[2] They are also said to be white, hairy, and extremely fond of honey—points which naturally suggest that they are really snow-bears. It must be remembered, however, that bears are quite as familiar a sight to the natives as omnibuses to the Londoner.

[3] One of the fattest men who ever lived (1770–1809). He weighed 739 lb.—52 stone 11 lb.

and abnormal type of manhood was produced, and I learned that it was by the practice of eugenics *in excelsis*. The wrestlers are selected in boyhood from parents of unusual size; they are dieted and treated from the earliest years; as they grow up and enter the ring they are attended by a special bodyguard of masseurs, trainers, barbers, clothiers, and cooks; they are encouraged to consume an incredible amount of strength-producing food; and they constitute a separate guild, graded, numbered, and registered according to their capacity."

There is no doubt that careful breeding can accomplish wonders—for example, I believe that the mastiff and the pug (both, of them, alas, breeds which have been ousted from favour by the Chow, the much-maligned Alsatian, and the Pekingese) have been developed from a common strain—but it is difficult of application to human beings. Still, we may yet see giants bred in small numbers for show purposes—in which case Bishop Berkeley's (alleged) artificial giant[1] will no longer be unique among white men.

But I question whether there is, or has ever been, a race of men averaging, say, 8 feet or more in height. It is true that isolated cases of greater stature have occurred, but they are the result of accidental disease, and not of heredity. The giant of the dime-show and the music-hall is of interest to medical men with an eye to the dissection of his pituitary gland, and in their sight he may be a

[1] It used to be related that the great and good George Berkeley, Bishop of Cloyne, better known for his idealistic philosophy and his unequalled power of writing dialogue, had succeeded, by a system of dietary (on the lines of Wells' "Food of the Gods"), in making an unfortunate child grow into a giant. The true facts are these. A peasant boy named Cornelius Magrath, suffering from gigantism and half-starved for lack of food, was brought to the notice of Berkeley, who nursed him back to comparative health in his own house, and afterwards befriended him in many ways. Magrath died in 1760, aged 24. His skeleton (now 7 feet 7 inches) is in Trinity College, Dublin.

desirable specimen. But he is a poor specimen of humanity; disproportionate,[1] feeble, ailing, and with acromegalic hands, jaw, and feet. Machnov (9 feet 3 inches), who possessed some of these stigmata in a marked degree, could never have been taken for a normal man, even in a photograph which gave no indication of scale. Such giants die young;[2] and if they beget children (which is seldom), these, more fortunate, are usually of normal size.[3]

As Domain, maker of robots, remarks,[4] "Our planet is too small for giants." There is a structural limit to the useful size of all creatures built of flesh and bone. A man's weight varies, roughly, as the cube of his height; but his strength (which depends on the cross-section of his muscles) as the square of it. If, say, a man of 5 feet 10 inches were to be increased by one-seventh all round, he would become about half as heavy again, but he would only be stronger in the proportion of 64 to 49, so that he would actually be less efficient, physically, than he was before—and, both now and always, the future is to the efficient. The huge beasts and birds of prehistoric times, and some which survived till a quite recent day, died out because, even if Man spared them, Nature would not. And so, even if it be true that "there were giants in the

[1] Chang Wow Gow (1845–93) is, I believe, the only case on record of a man 8 feet high and perfectly proportioned. He was a splendid specimen of humanity and extremely intelligent, speaking six languages. At the Prince of Wales' request, he once wrote his name on a wall at the height of over 10 feet from the floor.

[2] In almost every case, before thirty. Chang died at forty-eight; M. C. Miller (7 feet 10 inches) at sixty. I believe that an American giant, Captain M. V. Bates (who stood about 7 feet 10 inches and his wife 7 feet 9 inches), lived to be over fifty; but I am not certain of this.

[3] With regard to the children of giants, it should be noted that J. R. Forster, who sailed (and squabbled) with Cook on his second voyage round the world, records that the average height of the citizen of Potsdam was unusually great, a fact which he ascribes to their frequent intermarriages with the soldiers of the King of Prussia's "giant regiment".

[4] In Kapek's *R.U.R.*—a strange play which has given a new word to the English language.

earth in those days,"[1] it is probable that their descendants have long since joined the Dodo, the Mammoth, the Moa, and Steller's Sea-cow in whatever limbo is reserved for Nature's discarded experiments.

[1] Genesis vi. 4.

V

BEALINGS BELLS

On March 1, 1834, there appeared in the *Ipswich Journal* a long and remarkable letter. It was from Major Edward Moor,[1] F.R.S., and described the extraordinary disturbances which had occurred, and were still occurring, in his house at Great Bealings, Suffolk.

An occasional "runaway ring" is not very uncommon, especially when small boys are about; but at Great Bealings peals of four or five house-bells would often be rung simultaneously (and with extraordinary violence) while a watch was actually being kept on them, and when it seemed absolutely out of the question that this could have been done by any human agency. These peals, and occasional single rings, continued to occur at frequent and irregular intervals during a period of some seven weeks—and, in spite of repeated efforts, no definite cause for them could be discovered.

The *Ipswich Journal*, not unnaturally, opened its (fortnightly) columns to further correspondence on the subject; and to this Moor (whose good faith was unquestionable) contributed, from time to time, further bulletins as to the progress of the disturbances. And when these had finally stopped, and the matter had become a mere nine days' wonder, he devoted his leisure (of which he seems to have had a good deal) to further investigations. He had already confessed himself baffled as to the cause of the bell-ringing in his own house, and to this he

[1] A well-known writer on Hindu mythology. He was born in 1771, and served in the East India Company's forces from 1782 to 1806, when he retired, with a special pension for his distinguished service. In character, he seems to have been a more prudent Colonel Newcome. Edward ("Omar") Fitzgerald and he were old friends. He died on February 26, 1848, aged 77.

gave no further attention; but he made it his business to track down, as far as possible, the particulars of any other similar cases.

He seems, when he began his researches, to have been under the impression that the bell-ringing at Great Bealings was an entirely novel phenomenon. To him it probably was. Nowadays, those who are well-read in such matters would probably classify the Bealings bells as a somewhat unusual, but not unique, variety of the manifestations generally associated with the "poltergeist" —that irritating person whom sceptics regard as being, invariably, a mischievous child or servant; while those more credulous consider him an "elemental", possessed of a somewhat crude sense of humour.

During 1834–41 Moor accumulated details of some thirty cases which he regarded as similar to his own, and in the latter year he published his results in a small book entitled *Bealings Bells*.[1] The motive for its publication was, avowedly, the very common one of trying to make a little money—but for a "worthy object". A new church was to be built at Woodbridge, about five miles east of Great Bealings, and it was hoped, with the usual optimism, that the present "scanty funds" would be augmented as the result of a bazaar. *Bealings Bells* was written for sale at that dismal function. I have no information as to the result, although this can perhaps be inferred from the fact that the book is extremely rare.

Such being the case, it seems worth while to compile from Moor's monograph—which, after all, is the *locus classicus* of a singular and rare phenomenon—an account of what happened at Great Bealings, and an outline of some of the other cases collected by him. A good deal

[1] BEALINGS BELLS. // An Account / of the / Mysterious Ringing of Bells / at / Great Bealings, Suffolk, / in 1834; / and in other parts of England: / with Relations to farther / Unaccountable Occurrences, / in / Various Places: / by / Major Edward Moor, / F.R.S. &c. // Woodbridge: / Printed and sold by John Loder, / For the Benefit of the New Church. // 1841. (12mo. xiii+142 pp.).

of selection and compression is necessary. The book is not very well arranged, and details are alternately piled and withheld in a most exasperating manner. What conceivable purpose, for example, is served by beginning a chapter[1] like this?

"No. 29. THE —— HOUSE GHOST

"The facts I am about to tell, belong to —— House — or ——, as it was formerly called; a respectable old manor house in the north-eastern part of ——shire. It was, in very early times, the seat of the ——; a family of some distinction in the County."

Some of the cases cited as analogous, too, are quite otherwise. Moor solemnly sets down particulars of two cases in which public clocks have struck more often than they should, and ascribes this to the same mysterious agency which pealed his own house-bells. We are also told of a quite ordinary "haunted house" at Windsor (strongly suspected to be a fraud); of a poltergeist at Sydersterne, Norfolk; of the apparition of a man on horseback, both "of colossal stature" and seeming "as if they had been flayed alive", which appeared to two Army officers out shooting; and of mysterious thuds, like those of a pile-driver, which were at first thought to be produced by beetles. In the last-named case the reader's natural curiosity as to their species is gratified by a footnote: "Not the insect; it is believed—but the implement.—E.M."[2]

Even when dealing with the disturbances in his own house, Moor is sometimes a little difficult to follow. For example, he gives the most meticulously detailed description of the arrangements of the bell-wires; yet it is, so

[1] *loc. cit.*, p. 112.

[2] An engineer friend of mine once contributed to *The Times* an account of the demolition of a lighthouse, concluding with the statement that the last few stones had been removed by crabs. This excited so much astonishment that he was compelled to explain, in a later issue, that they were steam-crabs.

far as I can judge, quite impossible to construct from this description a diagram showing that arrangement. It may be as well to point out that his bells, like those in every case which he describes, were of the old-fashioned kind, hung on spiral springs, and rung by pulling; this pull being transmitted from the "bell-pull" by means of thin wires led either through detachable casings secured to the walls of the house, or inside the walls themselves. The direction of the wires was changed where necessary by "bell-cranks"—L-shaped levers, pivoted at the angle of the L, and having the wires made fast to the two arms. Drawing out the bell-pull[1] about half an inch is usually enough to ring all such bells; on releasing the pull, a spring returns the wires to their original position.

But while it is easy to censure Moor's book as badly arranged, and its author as pedantic—and, in spite of the letters after his name, unscientific—there is another side to the picture. He wrote at a time when such subjects were not taken very seriously; he took a great deal of trouble to investigate various recondite matters which, but for his labours, would not have been permanently recorded; he may not always, or even frequently, get hold of the right end of the stick, but it is his own painstaking record of his labours that enables this to be seen; he is perfectly frank and obviously honest. And he has a very remarkable story to tell.

Returning from church on the afternoon of Sunday, February 2, 1834, he was told that the dining-room bell had been rung three times between two and five o'clock. There was no one then in the room, and no apparent cause for the ringing. On Monday, much the same thing occurred. Moor considered that birds must have shaken the bell-wires—these, at one end of the house, ran outside it, and close to a pear-tree much frequented by blackbirds. On Tuesday the 4th, returning home about 5 p.m., he

[1] Bells rung from outside a house generally have horizontal bell-pulls: inside the house the wires are usually pulled by a crank-handle.

learned that five of the nine bells hung in the kitchen had been ringing peals, at intervals of about a quarter of an hour, for the last two hours.

With his son, he visited the kitchen, and while he was looking at the bells, they rang again, very violently—so much so that, although he knew what to expect, he was greatly startled. Ten minutes later, the same thing happened again, not quite so violently; and a third peal followed some fifteen minutes later. This went on, the interval between the peals gradually increasing, until a quarter to eight, when the disturbance stopped, except that about an hour after silence had fallen an attic bell, hanging by itself in the kitchen, sounded gently.

For a period of fifty-four days[1]—February 2 to March 27, 1834—these strange ringings continued. Their character was more or less uniform—peals of five bells, or of a different three, varied by occasional rings from the single bell mentioned previously, or from another single bell hanging in an attic, and connected with a bedroom. A curious feature of the peals was the extreme violence with which the bells were agitated—this could not be duplicated by ringing them from the bell-pulls in the ordinary way.

Moor does not state whether the bell-pulls and/or the wires (which could be sighted along practically their entire extent from pull to bell) were observed to move when the bells were pealed. But he is careful to point out that, so far as he could judge, no human agency could have produced what he heard.

"The bells rang scores of times when no one was in the passage, or back-house, or house, or grounds, unseen. I have waited in the kitchen, for a repetition of the ringings, with all the servants present—when no one—hardly 'so much as a mouse'—could be in concealment. But what matters?—neither I, nor the

[1] Moor, in a footnote to p. 68 of his book, calls this fifty-three days.

servants, singly or together—nor any one—be he whom he may, could or can, I aver, work the wonderment, that I, and more than half a score others, saw. . . ."[1]

"I will here, note, once for all[2]—that after much consideration, I cannot reach any procedure by which they (these effects)[3] have been, or can be, produced.

"If I had a year to devote to such considerations, and the promise of a thousand pounds in the event of discovery, I should despair of success. I would not, indeed, attempt it."[4]

At the time of the ringings, he had stated, in a letter to the *Ipswich Journal*:

"*February* 25, 1834

"P.S. I had on the above date (25.ii.34) an opportunity of reading the above proceedings . . . to six or eight very intelligent gentlemen at Woodbridge—and add, as my answer to some of their queries—that I keep no monkey—that my house is not infested by rats—that the wires of the five, and of the three, *pealers*, are visible in their whole course, from their pull to the bells, save where they go through walls, in which the holes seem no bigger than is necessary. The wires of the two single bells are also visible, except where they go through floors or walls. One or two of my friends, said it was '*all a trick*'. It is *possible*. I have for many years of my life passed over large arcs of the earth's surface, and have seen divers tricks of distant people. If this be one, it surpasses all that I have seen. . . ."

Needless to say, the theory mooted by his friends had

[1] Pp. 17, 18.

[2] The punctuation is that of the original: I am inclined to blame the printer, who seems to have followed the old tradition of "three stops to every line".

[3] This is interpolated.

[4] P. 21.

several supporters—notably a correspondent[1] of the *Ipswich Journal* who appears, from his letter,[2] to have been a retired acrobat living in a house designed by the late J. N. Maskelyne.

> "In my house, which compared with Mr. Moor's, is of limited extent, a person may, in a second, set three bells ringing violently by touching the wires, which all pass along a passage and through a hole in the wall, and in less than two seconds may get out of the passage at any one of the five doors, and may almost instantaneously set other bells ringing, and within two seconds may enter the kitchen in a different direction from that of the passage where the three bells were set ringing."

He suggested to Moor, however, a very sensible plan of action—namely, to send for a few trustworthy neighbours, seat his entire establishment in one room with a friend, lock them in, station a friend on each staircase, and search the house, locking each room as he proceeded. "If this plan be pursued I will . . . make any moderate bet, either that the bells will not ring at all during the search, or that, if they do ring, the party searching the house will find some relative, or friend, of one of his establishment concealed in some part of the house."

It would certainly have been more satisfactory if this had been done. But the letter did not commend itself to Moor. He remarks, rather loftily: ". . . I did not in any way, follow the advice therein offered." It must be remembered, though, that he had previously satisfied himself that the bells could not be rung from any of the rooms in a way which reproduced the violence of the mysterious peals; and he also ascertained that they could not be so rung even by tampering with the

[1] One still sees his name in the papers. He signs "Constant Reader".

[2] *loc cit.*, p. 11.

wires at a point intermediate between the bell-pull and the bell.

To do this he tried the effect of pulling down with a hoe the wires of the five "pealer" bells at a point where they all ran parallel, and close together, along a passage between the kitchen and the back-house. By his description, it seems to have been about the most favourable spot at which to pull if one wished to disturb the bells—although no one could do so without being observed from the kitchen. The effect of his test was to show that if he pulled the wires violently the bells did not ring at all; while if he pulled them gently, the bells would tinkle, but no more. He admits, though, that it took less force to set the bells in motion than he would have expected.

He also admits that there is one point, that at which the five wires first converge, from which they could be made to ring.

> ". . . A rope led horizontally . . . and so pulled would ring them. But they could not be rung with the violent jerks witnessed and described. With no vigour of pull could that be effected. The cranks and wires might be broken: and I have no doubt but they would be, with very much less violence of pull. This is the only mode that occurs to me by which I could, even gently, ring the four or five bells. I confess, however, that the ringing in the passage, by the direct downward pull, moved the whole six bells into sound, easier than I had foreseen, or expected, before trial.[1]
>
> "But it does not shake my expressed conviction—that my bells were not so rung by any mortal hand."

He is very non-committal as to any possible explanation; he gets no nearer to one than this extraordinary rigmarole:[2]

[1] He afterwards ascertained that no downward pull, applied anywhere along the whole run of the wires, could do more than tinkle the bells.

[2] *loc. cit.*, pp. 57, 58.

"The question ever recurs—what can be the cause? An adequate cause must exist; for these effects, and for every effect; moral and physical, in nature. But, in this case, no one has yet pretended, so far as I know, to develope [*sic*] it.

"It may be no advance to say—that, *possibly*, some hitherto undiscovered law of electricity or galvanism—latent—brought into activity, only by certain combinations of metallic alloys, in certain co-extension of parallelisms, straightness, or angles—certain concurring, or varied, degrees of tension—in connection with certain conditions of atmospheric influences—acted upon by agencies, subtle and occult, &c., &c.

"These possibilities—whose combined eventualities may, or may not, ever be developed—may be only another link in the amazing chain of results, that recent researches into the mysterious operations of science and galvanics, have brought under the wondering eye and contemplation of chymical philosophy.

"Who can say, or imagine, where they are to end? . . ."

There is a good deal more of this sort of thing, which may be all very well for the wrapper of a patent medicine, but seems a little out of place in a book written by a Fellow of the Royal Society.

I turn to the cases collected by Moor. He omits—whether designedly or otherwise there is nothing to show—the bell-ringing at Sampford Rectory in 1810, of which an account was published by "Lacon" Colton, who then held the living.[1] Some of his instances, too, are so shorn of detail by excisions that they are of no value; in one or two cases, no details seem to have been vouchsafed. The following, for example, might as well not have been printed:

[1] *A Plain and Authentic Narrative of the Sampford Ghost* (1810). There is a copy of the Appendix to this in the British Museum, but none of the pamphlet itself. Colton's bells rang all night, apparently untouched.

"No. 19. A gentleman, whom I do not know, called on me, and stated that his father took a house in the country, that had been a lady's school.—The dinner-bell frequently rang during the night; and the bells in the house, were frequently ringing.—He tried an experiment, by fixing a bell to the wall; and it rang. . . ."

Two of the cases, however—the ringings at Greenwich Hospital in 1834 and at Stapleton, near Bristol, in 1836—deserve mention.

The Ringings at Greenwich

The best authority for the Greenwich ringings is a letter from Lieutenant William Rivers, R.N.,[1] in whose rooms they occurred, to Major Moor. His account is supported by a very similar one written by his wife, and by contemporary newspaper reports. I may say that I heard something of the story, as a still-lingering tradition, when I was at Greenwich Hospital (now Greenwich Naval College) in 1911.

(*Rivers to Moor*)[2] GREENWICH HOSPITAL

April 26, 1841

"The bells in my apartments in Greenwich Hospital, from some unknown cause, commenced ringing at half-past six o'clock, on the morning of the 30th September, 1834; and continued, first one, and then another, at intervals of four or five minutes, and sometimes all four at once. The first day, I had a minute examination

[1] Rivers (*b.* 1788) had served in the *Victory* at Trafalgar, where he lost a leg. He was Warden of Woolwich Dockyard from 1824 to 1826, when he became a Lieutenant of Greenwich Hospital. He died there in 1856.

[2] The punctuation of this letter, and Mrs. Castle's, departs so widely from accepted rules that I have had to alter it. The printer appears to have run short of commas, and to have used semicolons instead (*loc. cit.*, pp. 81–3).

made by the clerk of the works, and the bell-hanger; and in the evening, at eight o'clock, I had the wires cut off from them. The bells then ceased to ring; but the wires were agitated for some minutes afterwards. All remained quiet during the night. At nine o'clock next morning, the bell-hanger came, and re-united the wires to the bells; which had no sooner been done to the first, when it rang; the second the same; and they continued at intervals, as before, all that day: and many persons witnessed the performance.

"In the evening, about eight o'clock, I tied up the clappers; while so doing, the bells were much agitated and shook violently. They ceased to ring during that night. In the morning I loosed them again; when they began to ring again. The clerk of the works, his assistant, and Mr. Thame the bell-hanger came and had another examination, without discovery as to the cause. They requested the family and servants would leave the apartments to themselves. We did so, and dined at four o'clock at our neighbour's opposite; and while at our dinner there we heard the bells ring a peal. Mr. Thame, and the assistant to the clerk of the works, remained until eleven at night, one watching the cranks, the other the bells below, with perfect astonishment: but they[1] ceased at their accustomed time, about nine o'clock or half-past. At eleven o'clock I requested them[2] to retire, having made up my mind to sleep there by myself; but my brother-in-law, Capt. Watts, and my wife, determined likewise to do so. I searched the apartment before I went to bed, and retired at half-past eleven o'clock. In the morning they began to ring again; but more faintly than before. I was then fully resolved to let them have their play out. . . . [They stopped about 3 p.m. on Friday, October 3rd.]

"I must here mention, that what appeared most extraordinary was the movement of the cranks, which

[1] The bells.

[2] Thame and the other man.

(the bell-hanger said) could not cause the bells to ring without being pulled downwards; which they did, of themselves, in every room, working like pump-handles. . . ."

The Ringings at Stapleton

The account which Moor prints of the Stapleton case[1] is contained in a letter from a Mrs. Castle (who, with her husband, was living in the house where the ringing occurred) to a Mrs. Shawe, of Kesgrave Hall, Woodbridge, by whom it was forwarded to Moor.

STAPLETON GROVE

May 8, 1841

". . . One afternoon in July 1836, the bell of one of the sitting-rooms was observed to ring loudly several times; no person having touched it. In the course of half an hour the same thing occurred with nearly (if not) every bell in the house. Sometimes one would ring singly; then three or four together. The wires were distinctly seen to descend, as if pulled violently.

"I sent for the bell-hanger; but before he arrived, the noise had ceased. He examined all the wires, without being able to discover any cause for this singular occurrence; and was about to take his leave, as it was growing dusk, when the bells again began to ring more violently than before. One we particularly noticed at this time, belonging to a room immediately over the passage in which the bells hang. It is pulled by drawing up a little slide against the wall; and the wire merely passes through the floor to the bell below. The slide we watched for more than five minutes. It was constantly shaken; even making a rattling noise, and the bell ringing.

"When it had continued about an hour, I desired the

[1] *loc. cit.*, pp. 97–9.

bell-hanger to take down every bell, as our only chance of passing a quiet night. The maid-servants (who, as you may imagine, were a good deal alarmed) assured me that the wires continued to shake through the night; but I cannot vouch for the correctness of this statement, and think it was probably a little fancy on their parts. The weather was rather hot, but we were not aware of any thunder during that week.

"I think it impossible that there could have been any trick, as I assembled all the servants in one place, and had the house thoroughly searched. The bells had all been newly hung[1] about twelve months before with stout copper wire. They were all replaced the next morning, and have never shown a disposition to be riotous from that time. . . . We have always supposed it to have been caused by electricity.

"MARY CASTLE"

In all such cases—and, indeed, in all disturbances of the poltergeist type—there must be a very strong presumption that the whole thing is the result of human agency. If a servant, or a child, has a grudge against the occupants of a house—or if any one desires to unsettle them, or to induce them to quit it—their bells offer a simple means (at least, for a time) of causing them a maximum of annoyance at a minimum risk to one's self. Nowadays, of course, the best method is to keep on ringing them up on the telephone—although if one takes the obvious precaution of using a series of call-boxes it is likely to prove a somewhat expensive amusement. But in the days before telephones, or even electric bells—and the Bealings, Greenwich, and Stapleton cases all occurred between 1834 and 1836—the facilities which house-bells offered to persons desirous of paying off a score of any kind were certainly second to none.

[1] i.e. the operating wires renewed. The bells are actually hung on spiral springs, formed from flat steel strips about an inch wide.

If the disturbances at Bealings, Greenwich, and Stapleton had been of short duration, there could be little doubt that each was either a practical joke or a piece of spite. It is their duration which makes them remarkable. The longer they went on, the more attention they would attract; and, consequently, the greater would be the risk of the operator being discovered. Besides, familiarity, as we all know, breeds contempt; if he did not effect his purpose in a short time, he was not likely to do so by persisting with what had ceased to be a startling and unnerving thrill, and had become merely a nuisance, to be inquired into and, if possible, abated. It is easy to imagine many ways in which a fraud might have been perpetrated for a short time; but it is difficult to conceive why it should have been persisted in for so long—and still more so how the *modus operandi* failed to be discovered once curiosity was thoroughly aroused. Moor, as we have seen, was most thoroughly convinced that no human agency moved his bells; the minions of the Board of Works were equally baffled at Greenwich; Mrs. Castle took effective measures which certainly should have exposed a mere trick.

As the Senior Wrangler remarked of the *Ode to a Nightingale*, such occurrences, however well authenticated, "prove nothing" and lead nowhere. They may have some natural explanation—most probably there is one, if we knew exactly where to look for it. On the other hand, it is an indisputable fact that the persons chiefly desirous of finding such an explanation, who may be presumed to have known more of the facts than we can hope to do now, were not successful. If Major Moor and the others were merely the victims of a prolonged and annoying hoax, its perpetrator may at least be congratulated upon having outshone Cagliostro, the Davenport Brothers, and many "mediums" of our own day—in that he went to his grave undetected and unexposed.

Postscript

In his *Recollections of a Geographer* (London, 1935) Mr. E. A. Reeves relates a personal experience (*c.* 1890) which closely resembles the Stapleton case, and which proved equally insoluble.

Another case occurred during the War. The bells all over an old house in the heart of Greenock developed a habit of ringing, without apparent cause, in the middle of the night. Investigation revealed that Diesel engines for submarines were being tested, by night, at a neighbouring factory, and that the vibration thus produced, while imperceptible to human beings, was enough to set the bells in motion—their "period" of swing happening to coincide with it. But no explanation of this kind will cover the sudden, appalling peals heard at Great Bealings, or the independent movement of the bell-pulls at Greenwich and Stapleton—quite apart from the fact that at the epoch of those ringings there were, so far as I can discover, no factories in operation anywhere in the vicinity.

VI

THREE STRANGE SOUNDS

The Cry of Memnon

Still from his chair of porphyry gaunt Memnon strains his lidless eyes
Across the empty land, and cries each yellow morning unto thee.

If Tennyson, as seems likely, excelled Wilde as a poet, he nevertheless showed himself inferior to the author of *The Sphinx*[1] in accuracy when he wrote:

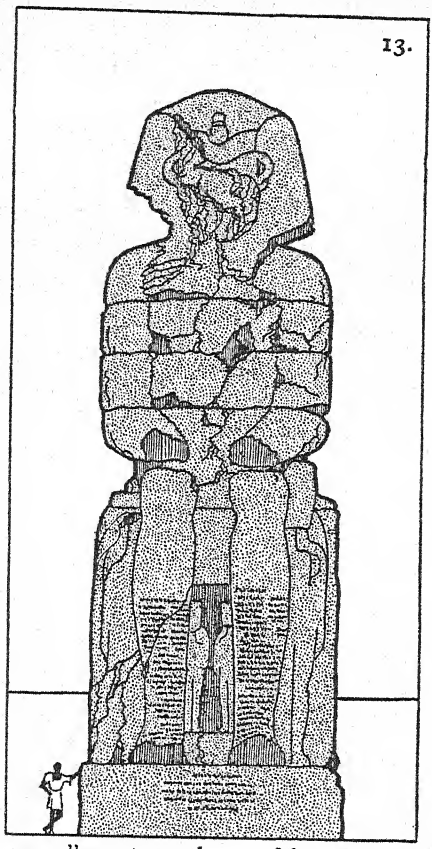
> . . . from her lips, as morn from Memnon, drew
> Rivers of melodies.

The sounds which are recorded as having been emitted by the famous "vocal statue of Memnon" were neither many nor melodious. Infrequently, but always at sunrise, those who stood near it long ago might hear a thin, strident sound, like the breaking of a harp string. That was all—an aimless cry heard at rare intervals during a relatively short period of two hundred years, a period preceded and followed by many centuries of silence. Yet it was a phenomenon of which hardly any similar case is on record—and it was not, it should seem, a deception. The statue, which had been silent for so long, and has again sunk into silence, did once acquire and exercise some strange inherent power of saluting the sun.

The statue of Memnon—which, incidentally, is not the statue of Memnon, but of Amenophis III or Amenhotep—was erected about 1500 B.C. Its architect (we can hardly, *pace* Mr. Epstein, call him a sculptor) was, rather confusingly, also named Amenhotep, son of Hapu. It forms one member of a pair of twin colossi, still standing,

[1] The initial quotation is taken from that bizarre poem—one of the few really original pieces that Wilde ever wrote. It will be noticed that the metre, though not the form, is that of *In Memoriam*.

about a mile from the western bank of the Nile, among the ruins of Thebes.



13. "Memnon"—eastern colossus of Amenhotep, at Thebes.

Here is their creator's own description of them:[1]

"For my Lord the King was created the monument of sandstone. Thus did I, . . . causing to be made

[1] From an inscription on his own statue, now in the Boulak Museum at Cairo.

two images of a most noble hard stone, in his likeness, . . . wonderful for their breadth, lofty in their height, the stature whereof made the gate-tower to look small. Forty cubits was their measure. In the glorious sandstone mountain[1] wrought I them, on this side and on that, on the east side and on the west. Furthermore, I caused to be built eight ships, whereon they were carried up. . . ."

Even in their present mutilated and defaced condition, the two colossi are a most impressive sight. Each is some fifty feet high,[2] and there is about the same distance between them. They sit side by side, looking SSE. towards the Nile.

The western figure, a single piece of stone, is featureless, and the breast, legs, and feet are badly damaged. The eastern, which is the celebrated vocal statue, has obviously suffered far more extensive mutilation. From the pedestal to the waist it is a single block, extensively cracked; from the waist upwards it is composed of five tiers of lighter stone, as if foreshadowing that puerile monstrosity, the 100-foot "statue of Columbus" recently constructed at Palos. As with its fellow, the breast is damaged and the features unrecognizable.

The material of the western statue, and of the lower part of the eastern, is stated[3] to be a "coarse hard gritstone". The upper part of the eastern statue is of sandstone.

As already remarked, the eastern or reconstructed statue is that which is credited with having once emitted sounds. Before discussing this question, however, it may

[1] Obviously, from the context, some quarry down river—possibly that of Toora, near Cairo.

[2] Wilkinson and Curzon, both writing from personal observation, differ in the heights which they assign. Wilkinson made the western statue, by sextant angles, 47 feet high, and the eastern (the vocal statue) 47 feet 9 inches, by actual measurement. Curzon gives 51 feet for each. Both sets of dimensions are exclusive of the pedestal.

[3] Wilkinson.

be as well to subjoin a short outline of its history (so far as that is known or conjectured)—a personal selection from the opinions expressed by several experts, often at variance.[1] The dates are approximate.

1500 B.C.	Erection of the statues.
524 B.C.	The statues defaced by Cambyses.[2]
27 B.C.	Upper half of the eastern statue thrown down by an earthquake, which also caused additional damage to the western statue.
20 B.C.	Strabo, the historian, visited Thebes, and recorded the fact that, both by report and his own observation, the eastern statue emitted a sound at sunrise.
19 A.D.	The sound heard by Germanicus.
90 A.D.	The sound heard by Juvenal.
130 A.D.	The sound heard by the Emperor Hadrian, and by Pausanias.
196 A.D.	Last recorded occasion on which the sound was heard.
(later)	Eastern statue reconstructed.

The dates on which the sound was heard, as given above, are only intended to provide a few points of reference. During the period 20 B.C.–A.D. 196 it seems to have occurred quite frequently, if irregularly—certainly not every morning, but probably several times a year at

[1] The literature of the subject is quite extensive. In the short sketch here given I have relied chiefly upon the following:

La Statue Vocale de Memnon. J. A. Letronne (Paris, 1833).
Modern Egypt and Thebes. Sir. G. Wilkinson (London, 1843).
The Voice of Memnon. Lord Curzon of Kedleston. This originally appeared in *The Edinburgh Review* for July 1886, and was reprinted, with slight alterations, in Curzon's *Tales of Travel* (London, 1923).

[2] Cambyses, King of Persia, subjugated Egypt about 525 B.C., and ruled it for some five years, during which time he is credited with having done an almost incredible amount of wanton damage to its temples and monuments. Possibly, like Cromwell, he may have been a dog with a bad name.

least. The authorities as to the simple fact of it being heard include Strabo, Pliny (not from personal observation), Pausanias, and Juvenal; the last of whom speaks of "dimidio Memnone"—*halved* Memnon, indicating that the statue had not then been restored. But, in addition to these, there is a considerable body of testimony inscribed on the statue itself. Its feet exhibit a series of eighty-seven inscriptions[1] in Greek and Latin (there are none in Egyptian characters) made by visitors who, like modern trippers, were moved by the desire to carve their name on something, and who took the opportunity of recording that they had, or had not, heard the cry of Memnon. These inscriptions range over a period of about two centuries. The last was made in A.D. 196.

As to the nature of the sound, Strabo speaks of it as resembling the sound of a slight blow. Pausanias states that it can only be compared to the sound made when the string of a lyre breaks. One of the inscriptions describes it as a high-pitched note, like that produced by striking brass.

The inscriptions contain some thirty-nine references to the time of day at which the sound was heard. Tabulated, the results are as follows:

Before sunrise	2
At sunrise	18
Less than an hour after	8
An hour after	6
Less than two hours after	2
Two hours after	3

The months most frequently mentioned are February and March, probably because they were those in which visitors to the statue could most easily ascend the Nile. Several of these heard the sound more than once—on

[1] These were all copied by Letronne, and are illustrated in his monograph. See also fig. 13.

the other hand, two inscriptions record success only at the second visit; and one, not until the third.

It is not absolutely certain, but almost so, that the statue has been silent since about A.D. 200 or so. There is one recorded instance of its having sounded in quite modern times; but this event is attested in so curiously roundabout a fashion that its value as evidence is exceedingly slight. Here is the original authority, verbatim. (*Revue Encyclopédique*, Tom. IX, p. 598. Paris, March 1821.)

> "EGYPTE.—*Antiquité.—Statue de Memnon.*—Dans une lettre adressée à l'ambassadeur de Russie à la cour de Rome, sir A. Smith, voyageur anglais, qui est maintenant à Thèbes, dit avoir examiné lui-même, avec sa nombreuse escorte, la célèbre statue de Memnon. A six heures du matin, il a entendu très-distinctement les sons dont il a été parlé si souvent dans l'antiquité. *Memnonis saxea effigies, ubi radiis solis icta est, vocalem sonum reddens.* Tac. ann. 2. M. Smith assure que ce bruit mystérieux ne sortait pas de la statue, qui a été renversée par un tremblement de terre, mais du piédestal; il le regarde comme le résultat de la percussion de l'air sur les pierres de ce piédestal, qui, selon lui, sont disposées de manière à produire cet effet."

It is somewhat singular, but apparently true, that the Theban investigations of this distinctively named English traveller, "sir A. Smith", would have been lost to posterity had it not been for this short abstract, in a French periodical, of a letter written by him to a Russian friend resident in Italy. During the past century, Smith's visit has often been cited as proving that the cry of Memnon has been heard in modern times, but no one seems to have discovered any better authority for it than the *Revue Encyclopédique*. For my own part, although I have not been able to spare the ten years or so which

would be required to make a really exhaustive search, I have done my limited best to throw more light on the subject—without success. I have not succeeded either in identifying "sir A. Smith" or in finding any further account of his travels in Egypt.

It will be noticed that he is stated to have assured himself that the sound came from the pedestal—which is a squared socle, some thirteen feet high,[1] and in one with the lower half of the statue itself. It is curious that this observation, reported to have been made by the last man who heard the cry, is in close agreement with the account given by Strabo, who was the first. Speaking from his own experience (he visited Memnon in 20 B.C., in company with Aelius Gallus, the Roman governor of Egypt), he relates that he heard a sound, but "could not affirm whether it proceeded from the pedestal or from the statue itself, or even from some of those who stood near its base."[2] His last suggestion—that the whole thing was a trick—is, in itself, plausible; and it is quite possible that the sound heard (if it was heard) by Smith really emanated from one of his "numerous escort", anxious that he should not be disappointed; but, as will be seen, this hypothesis does not cover the events of the period during which the sound was of frequent occurrence. Wilkinson, who was firmly convinced that the sound was an imposture, makes no mention of Smith's visit at all, although writing twenty years after it had become generally known: he merely remarks, in a footnote, "More than one traveller has repaired to the statue before sunrise in hopes of hearing the sound", leaving it to be inferred that none succeeded in doing so.[3]

But, whatever conclusion may be formed as to the

[1] About 6 feet of this are above the sand and 7 below.

[2] Strabo, lib. xvii.

[3] A misprint, which I picked up in proof before this essay first appeared, is really too good to omit. The printer had made Wilkinson say, very appositely, "More than one traveller has repaired the statue before sunrise in hopes of hearing the sound"!

reality of the events associated with the mysterious Smith, there can be little doubt that the cry of Memnon was repeatedly heard, by quite a cloud of witnesses, during the first two centuries of the Christian era. I turn to the question of how that sound was produced. Opinion on this subject is as sharply and naturally divided as the original half of the statue itself; in which a fissure, running east and west, extends from the waist to the pedestal. Some hold that the sound was the result of human agency: in plainer language, a pious fraud—others regard it as a rare, but not unknown, phenomenon due to natural causes.

The hypothesis of fraud has, at first sight, a great deal to recommend it. The suggestion of imposture is inevitable. Here is a statue, of a semi-religious character, visited by crowds of superstitious and ignorant persons who have been drawn to it by the belief that it has miraculous powers. Where miracles are expected, miracles can usually be made to occur—witness the punctual and obliging[1] manner in which the blood of St. Januarius usually liquefies several times a year in Naples cathedral.

So far so good. But three questions remain to be answered, namely:

1. How was it done?
2. Who did it?
3. Why was it done?

To the first question there are several possible replies, some of which are more plausible than others. The sound may have come from some interested person who had stationed himself (possibly, like the more speculative type of bookmaker, with a few "minders" in attendance) near the statue—and it may have been produced by a small pair of cymbals, or an actual lyre, or some device of the kind; which, as any Customs searcher will testify,

[1] So much so that it found no difficulty in conforming with the alteration of the calendar effected by Pope Gregory XIII in 1582.

could easily be concealed in the thaumaturgist's clothing. Such is the simplest explanation. On the other hand, some have imagined that the sound was produced in the interior of the figure. Two French writers, Langlès and Salverte, worked out the details of an elaborate mechanism which, according to them, must have filled quite a large space in Memnon's interior.[1] Unfortunately, no trace of such mechanism or cavity remains.

A statement published by Sir Gardner Wilkinson, F.R.S., in 1843, was long regarded as having demonstrated not only the fact of fraud, but also the way in which this was effected. Here it is:[2]

> "In the lap of the statue is a stone, which, on being struck, emits a metallic sound, that might still be made use of to deceive a visitor who was predisposed to believe its powers; and from its position, and the squared space cut in the block behind, as if to admit a person who might thus lie concealed from the most scrutinous observer in the plain below, it seems to have been used after the restoration of the statue; and another similar recess exists beneath the present site of this stone, which might have been intended for the same purpose when the statue was in its mutilated condition."

As Curzon has pointed out, Wilkinson is at some pains to make his theory of imposture absolutely unassailable. He is not content with finding one suspicious recess; he produces two, one used when only the lower half of the statue remained, the other after the reconstruction. Furthermore, he "proceeds to narrate" that while he had

[1] See Salverte's *Des Sciences occultes* . . . (Paris, 1829). He provided for a system of hammers, striking on sonorous stones and driven by a water-clock. The machinery was let off by the expansion of a metal rod, on which the sun's rays were concentrated by a lens which the figure held between its lips. He does not seem to have thought of adding an aerial and a few loud-speakers.

[2] *Modern Egypt and Thebes*, vol. ii, p. 161.

considered, in 1824, that the "metallic sound" produced by striking his stone did not tally with the traditional sound of Memnon, he subsequently (in 1830) came across the inscription[1] which compared this to the sound made by striking brass. Fortified by this, he stationed some fellahin below, and tapped his "sonorous block" with a small hammer.[2] His audience, on being asked what they heard, replied, "*Ente betídrob e'nahás*": which is, being interpreted, "You are striking brass."

Unfortunately for Wilkinson's reputation, Curzon (who visited the statue in 1886) discovered, with the help of a ladder and a foot-rule, that his "sonorous block" was simply a stone which had fallen from the superstructure (which was not, apparently, in place when the sounds used to be heard) and had jammed in the natural fissure dividing the lower (original) half of the figure. Nor was this all. Wilkinson's "squared space" was simply the gap which was left in the superstructure when the stone fell, and his other "similar recess" was nothing but the fissure itself! Wilkinson had obviously distorted the evidence of his own eyes into conformity with his prejudices.

So far as is known, there is nothing in the structure of the figure which affords any positive evidence of fraud. On the other hand, it is quite possible that the sounds could have been made by some one standing near it.

The remaining questions—"who did it" and "why was it done"—can best be discussed jointly, for each involves the other. The obvious answer to them is "The Egyptian priests, to gain prestige and/or money."

Unfortunately, this is contradicted by the facts. There is nothing which would lead us to suppose that the Egyptian priests were any more averse to "working the oracle" than priests of all creeds, ages, and nations have generally shown themselves to be. But how comes it

[1] On the statue itself, by one Ballilla. See p. 124.

[2] As Wilkinson recalls, Strabo declared the sound to resemble that of a slight blow or tap.

that they should have so shamefully neglected their opportunities? During many centuries the statue stood, intact, in front of the splendid temple of King Amenhotep. There is no contemporary inscription—no vestige of evidence at all—that during this period, when the priests and their strange religion held full sway, the cry of Memnon was ever heard.[1] As was first pointed out by Letronne, there are no Egyptian inscriptions on the statue, and nothing to show that it was ever regarded by the Egyptians as miraculous. This was reserved for its Greek visitors, fifteen centuries after its erection, when Egypt was in Roman hands. The Greeks probably knew the statue as Memnon, because, if it sounded at sunrise, they would naturally associate it with the fabled son of the Dawn; but to the Egyptians it was merely Amenhotep—and, it would seem, not an object of worship at all.[2]

Even if it be supposed that the priests, or any other interested persons (finding that the statue was, or might be, worshipped as Memnon by alien visitors) decided to make it appear to be vocal, one would at least imagine that there would have been some method in their proceedings. One would, for example, have expected that the sound would be heard when, and as often as, best suited the convenience of its visitors. In particular, one would imagine that if it were visited by any exalted person, whom it would be impolitic to offend, the sound which that person had come to hear would most undoubtedly be heard. But the facts are quite otherwise.

[1] Juvenal, it is true, is responsible for the statement that "when mutilated by Cambyses, the statue which saluted both the sun and the king afterwards saluted only the sun". But such a statement, made hundreds of years after the event, is not evidence.

[2] Much ink has been spilt on the question of how the mythical Greek hero Memnon came to be associated with various statues and temples in Egypt. Wilkinson has pointed out that *Miamun* was a title of Rameses II. Either this name or Amenophis might easily have been converted into *Memnon* by credulous Greek immigrants, eager to find support for the Homeric legends.

If we are to judge by the inscriptions, many visitors never heard the cry of Memnon at all; many others must have had to make repeated pilgrimages before they heard it; and, while it is true that the statue apparently performed three times[1] for the Emperor Hadrian, it is equally true that it remained obstinately silent on the first occasion when his consort paid it a visit (a fact which, as is duly recorded on the statue by one of her ladies-in-waiting, enraged her exceedingly)—while the Emperor Septimius Severus never succeeded in hearing the sound at all. It is past all believing that those responsible for the supposed deception could have acted with such wanton disregard of the feelings of the great. Such behaviour presupposes an absolute lack of elementary common sense—it would have been that of fools, not knaves.

There is much more to be said for the opposition theory; indeed, except for the sake of completeness, such a proceeding savours of "blacking the chimney", for this theory is now generally accepted. On the supposition that the sound was a purely natural phenomenon, many of the difficulties connected with it—e.g., the varying descriptions of the sound itself and of the time of day when it was heard—become much less formidable. Such a phenomenon would almost certainly be irregular both in its nature and its recurrence.

The sound was only heard at or near sunrise; it varied in note and intensity; and it occurred, at irregular intervals, during a period which apparently began when the upper half of the statue was demolished (and the lower half split) and ended as soon as (but, possibly, before) the upper half was rebuilt. From these data it is difficult to avoid drawing the conclusion that the sound was caused by the sun's rays warming the cleft and truncated lower half of the statue; that it was produced by the unequal expansion of the two portions of this fractured

[1] On separate days. One or two of the inscriptions, however, record that the sound was heard twice on the same morning.

monolith—causing them to move, fractionally, one against the other; and that they no longer had free scope for this interplay when they were compelled to support the great weight of the rebuilt upper portion.

Although no exactly similar case is known, there is a certain amount of evidence which goes to show that in all probability the cry of Memnon was due to a natural cause. Jomard, Jollois, and Devilliers, three of the scientists who accompanied Napoleon's expedition to Egypt, heard a similar sound, at sunrise, on two occasions —once in the granite quarries at Syene and once in a temple at Karnak. They describe it (exactly in the manner of Pausanias) as resembling the sound by made a breaking string. In their opinion it was caused by air, occluded in the crevices of the stone and expanded by heat, making its way out. Rather singularly, they did not connect their experience with the sounds traditionally accredited to Memnon. These they regarded as fabulous. Their experience at Karnak was confirmed later by Brugsch, who was there in 1851 and also heard the sound in question.

To my mind, the theory of unequal expansion has more to recommend it than that of occluded air, although the latter was also put forward independently by Humboldt as explaining certain sounds, like the note of an organ, sometimes heard near sunrise at various points on the Orinoco.[1] If it be the true explanation, it is difficult to understand why such sounds should not be fairly common wherever stone of a suitable kind is exposed to changes of temperature. On such an assumption, for example, most of the other Egyptian statues and buildings should also emits sounds.

On the theory of unequal expansion, it is true, we can only escape from this objection by supposing that the sounds heard at Syene and Karnak came from stones which, like the statue, had also been fractured in a

[1] Humboldt did not hear these sounds himself.

somewhat similar manner. There is no evidence for or against this supposition.

In any event, it seems most likely that the cry of Memnon had its origin in the fissure dividing the truncated monolith, and that if the circumstances of the fracture had been slightly different the sound probably would never have been heard at all; just as it ceased when the self-adjustment of the two portions was disturbed by the superimposed load.[1] It must be remembered, however, that there is no actual evidence that the reconstruction of the upper portion synchronized with Memnon's renewed silence, since the date of the reconstruction is not known, although tradition associates it with the Emperor Septimius Severus (in whose reign the sound ceased).

Not only is the date of the reconstruction obscure,[2] but also its motive. It was possibly done with a view to propitiating Memnon, in which case it may well have been ordered by Severus (who, as already stated, paid the statue a fruitless visit), particularly since such a piece of work demanded resources of labour and material which only an Emperor could command. The tiers of the Roman work are an eloquent tribute to the engineering ability of Amenhotep, son of Hapu, and his men. In its original monolithic condition, the complete statue must have weighed some 1,200 tons; yet the Egyptians hewed it in the "glorious sandstone mountain", transported it many miles by water, and successfully erected it—all with the crudest of appliances. I do not imagine that many modern engineers would care to prepare an estimate for such a job, and it is no disgrace to the Romans that a similar feat proved beyond their powers. To reconstruct the figure even as it now stands was a very considerable achievement.

[1] I regard the "sir A. Smith" incident as "not proven."

[2] The left foot was certainly repaired after the date of Hadrian's visit (*c.* A.D. 130), for one of the cramps has been let in through the inscription recording this.

A minor point of interest in connection with the statue of Memnon is the fate of the missing upper half. It does not seem to be anywhere in the vicinity of the figure; and, weighing some 500 tons, it was scarcely likely to have been removed in one piece. On the other hand, if it were broken in the fall, or subsequently,[1] the pieces would seem to have been removed (for some unknown reason) before the reconstruction, or else deliberately rejected—for the new work is of distinctly lighter stone. One is almost tempted to believe that the Roman engineers found it, still whole, near by; and that, unable to replace it, they found means to break it up and remove it, lest it put them to open shame.

Parry's Cannon

The cry of Memnon is an example of a strange sound which is known, by name at least, to most people. On the other hand, the remarkable incident which occurred in the course of some experiments on the velocity of sound made by Captain W. E. Parry in 1822 seems to have been almost forgotten.

Parry, who had already made himself famous by reaching Melville Island, half-way to Bering Strait, while searching for the N.W. Passage in 1819,[2] spent the winter of 1821–22 at Winter Island (66° N., 83° W.). He was not the man to waste his time; and during this period of enforced inactivity he carried out, with the assistance of his officers, a very considerable amount of scientific research. The results fill a quarto volume of

[1] If it were deliberately broken up, this must have been an exceedingly difficult task. Wilkinson, however, speaks of a statue in the palace of Rameses II which he took to be Shelley's Ozymandias, and which was so utterly shattered that it almost seemed to have been blown up.

[2] By doing so he secured the Parliamentary reward of £5,000 offered in 1818 to the first explorer who, sailing westward to the north of the American continent, should pass the meridian of Long. 110° W.

432 pages;[1] and a notion of the diligence with which the work was done may be gathered from the fact that his astronomer, the Rev. George Fisher, took no less than 2,500 "lunar distances" (for determining longitude) in December 1821,[2] and an equal number in the following March.

Amongst his other researches, Parry made a series of experiments to determine whether the normal velocity of sound underwent any noticeable alteration at low temperatures. His opportunities for making them were excellent. The temperatures available ranged from about −45° to +40° Fahr., and he was able to use an absolutely ideal testing-ground. Between Winter Island and the mainland of Melville Peninsula, to the south-westward, is an arm of the sea—practically land-locked and (during the winter) frozen hard. Parry named it the Frozen Sea. Here, on eighteen days between December 29, 1821, and June 18, 1822, Parry made his experiments, from which he deduced the expected result that the average velocity of sound decreased slightly as the temperature fell, the amount of alteration being small and uniform. But on one occasion he encountered another deviation from the normal which, it is safe to say, he did not at all expect.

The experiments of February 9, 1822, were made as follows. A base-line slightly over a mile (5,645 feet) long was carefully marked out on the surface of the ice. At one end Parry mounted a six-pounder gun and its crew, with an officer in charge. Together with another observer (Fisher), he stationed himself at the opposite end of the base. The gun was given an elevation of about 10°, and pointed towards the observers. It is hardly necessary to add that it fired a blank charge. The temperature was −25° Fahr.

[1] *Appendix to Captain Parry's Journal of a Second Voyage*, London, 1825.

[2] It is sad to record that the mean of these, although they were all taken on shore, differed from that of the March observations by no less than 14′.

The procedure was as follows. The officer at the firing-point, when assured that the observers were ready, gave the order to fire the gun. Each observer noted independently, by a pocket chronometer,[1] the interval between seeing the flash of the gun and hearing the report. It should be added that on this and every occasion the experiments were made late at night, in order to avoid atmospheric disturbances. Both flash and report, therefore, could be noted with very fair accuracy.

Fifteen rounds were fired. The observed times agreed well, and gave a mean velocity of 1,023 feet per second. But Parry and Fisher were perplexed to notice that, on several occasions, the order "Fire", which was plainly audible (although uttered something over a mile away) reached them about half a second *after* the report of the gun. Here is Fisher's own statement, extracted from his official "Abstract of Experiments to determine the Velocity of Sound" (*loc. cit.*, p. 239):

> "The Experiments on the 9th February, 1822, were attended with a singular circumstance, which was—the officer's word of command 'Fire' was several times distinctly heard both by Captain Parry and myself about one beat of the chronometer[2] *after* the report of the gun; from which it would appear, that the velocity of sound depends in some measure upon its intensity. The word 'fire' was never heard during any of the other experiments; upon this occasion the night was calm and clear, the thermometer 25° below zero, the barometer 28·84 inches, which was lower than it had ever been observed before at Winter Island. Upon

[1] The pocket chronometer—a watch with the spring-detent or chronometer escapement—is now obsolete, on account of its liability to be stopped in the pocket by a shake or jar. Unlike a lever watch, it will not restart itself. Stop-watches were not in common use until long after Parry's time.

[2] This beat eight times in three seconds; an unusual and inconvenient arrangement. Most pocket chronometers have an "18,000 train," and beat five times in two seconds.

comparing the intervals between the flash and report of a musket with the gun, upon other occasions, there appears to be no assignable difference."

The exact meaning of the last sentence eludes me; it is not very clearly expressed. Fisher leaves the reader in doubt as to whether, on this particular occasion, comparative tests between gun and musket did show a difference; or whether no such tests were made on that occasion, but that they were made at some later date, and no difference observed. I assume that, whenever the tests may have been made, the object of them was to test Fisher's theory that the velocity of the sound depended on its intensity. That they gave entirely negative results is not altogether surprising; so far as I know, the phenomenon of "Parry's cannon" remains unique, not only in his experience but universally.

It is easy to be wise after the event. In the *Philosophical Transactions* for 1860 there appeared a learned paper on the theory of sound,[1] written by the Rev. S. Earnshaw. In this he lays down and elaborates the theory that, in certain circumstances, waves of sound have a power of propagating themselves, in advance, along the path which they are travelling. He continues:

"I should expect, therefore, that in circumstances where the human voice can be heard at a sufficiently long distance, the command to fire a gun, if instantly obeyed, and the *report* of the gun might be heard at a long distance in an inverse order; i.e. *first* the report of the gun, and *then* the word 'fire'."

(He next proceeds to lay a pitfall for the inquiring reader by referring, in a footnote, to Parry's singular experience, and citing the wrong authority for it.[2])

[1] "On the Mathematical Theory of Sound" (*loc. cit.*, pp. 133–48).

[2] He remarks: "See Suppt. to Appendix of *Parry's Voyage in 1819–20* . . ." There is no supplement to the published account of that voyage. "1819–20" should read "1821–23."

This is "all werry capital", but his theory does not square with the facts. The circumstances which he postulates, allowing the human voice to be heard at long distances, are not at all unusual in the Arctic. During Parry's third voyage, for example, Lieutenant Foster, one of his officers, found that it was perfectly possible to carry on a conversation across the frozen surface of Port Bowen Harbour[1] between the ship and a shore observatory, distant no less than a mile and a quarter. Similar acoustic conditions prevailed during the period of Parry's experiments on the Frozen Sea. Yet on no other occasion did the sound of the gun outrun the order to fire it—nor, as will be noticed, did this invariably occur even during the series of tests made on February 9, 1822.

Sir G. B. Airy, in his treatise on Sound,[2] suggested that the cause of the anomaly was physiological; that owing to the abnormal cold the observer's power of apprehending sound was affected, and less sensitive for some noises than for others. He suggests, in fact, a sort of delay-action between ear and brain, which affected the order more than the report. But this theory, also, is open to the same objection. The cold was not so great as that experienced in several of the other experiments; for example, those of February 16th (a week later) were made in −45° Fahr.—20° colder than it was on the 9th. In fact, the only distinctive circumstance on that date was, as remarked by Fisher, the abnormally low barometer.

A correspondent whose opinion I value has pointed out that sound travels faster in warm than in cold air; and that during Parry's experiments the stratum of air lying nearest the surface of the ice was probably the coldest. The order "Fire!" would travel through this stratum, and the sound of the gun—owing to the 10° elevation—through one somewhat higher. He admits, however, that the difference

[1] 73° 13′ N., 88° 52′ W.

[2] *On Sound, and Atmospheric Vibrations* . . . (London and Cambridge, 1868, pp. 134, 135).

in time ought not, theoretically, to be nearly so much as $\frac{3}{8}$ sec. I will add, that the simple supposition that what Parry and his men heard was a faint echo from some object about 200 feet in rear of them is ruled out by the fact that they must then have also heard the echo of the report itself.

The Barisal Guns

The Barisal guns have no affinity to Parry's cannon—in fact, it is quite certain that they are not guns at all. The term is used to denote certain remarkable sounds resembling gunfire, but certainly not such, which occur in many parts of the world—particularly the Sundarbans, or Sunderbunds, that enormous network of swamps and morasses through which the Ganges finds its way by many channels to the sea. Barisal itself is a village in the Sunderbunds, a little westward of the principal mouth of the Ganges and about 70 miles southward of Dacca.

At Barisal, and many other places in the Ganges delta, the guns are often heard. Here is one observer's account of them.[1]

> "I first heard the Barisal Guns in December 1871, on my way to Assam from Calcutta through the Sundarbans. The weather was calm and clear, no sign of any storms. All day the noises on board the steamer prevented other sounds from being heard; but when all was silent at night, and we were moored in one or other of the narrow channels in the neighbourhood of Barisal, Morelgunge, and upwards, far from any villages or other habitations, with miles and miles of long grass jungle on every side, the only sounds the lap of the water or the splash of earth, falling into the water along the banks, then at intervals, irregularly, would be heard the dull muffled boom as of distant cannon.
>
> "Sometimes a single report, at others two, three or

[1] A memorandum by Mr. G. B. Scott, quoted in *Nature*, 2. i. 1896.

more in succession; never near, always distant, but not always equally distant. Sometimes the reports would resemble cannon from two rather widely separated opposing forces, at others from different directions but apparently always from the southward, that is seaward. We were not very far from the sea when I first heard them, and on mentioning to an old lady on board that I heard distant cannon, she first told me of the mysterious sounds known as the 'Barisal Guns'."

Colonel H. S. Olcott[1] speaks of the sound heard by him, at Barisal itself, as being so sharp and loud that he thought it was the evening gun being fired at a cantonment in the village.

Mr. Scott, in the account previously quoted, also speaks of having heard similar sounds at Chilmari, on the Brahmaputra, about 300 miles inland.

"I specially remember spending a quiet Sunday, in the month of May, with a friend at Chilmari, near the river-bank. We had both remarked the reports the night before, and when near the hills previously. About 10 a.m. in the day, weather clear and calm, we were walking quietly up and down the riverbank, discussing the sounds, when we heard the booming distinctly, about as loud as heavy cannon would sound on a quiet day, about ten miles off, down the river. Shortly after we heard a heavy boom very much nearer, still south. Suddenly we heard two quick successive reports, more like horse-pistol or musket (not rifle) shots close by. I thought they sounded in the air about 150 yards due west of us over the water. My friend thought they sounded north of us. We ran to the bank, and asked our boatmen, moored below, if they heard them, and if so in what direction. They pointed south!"

[1] The Theosophist. See his letter in *Nature*, 12.xii.1895.

Similar sounds were heard on several occasions by Colonel Godwin Austen, of the Survey of India, still further inland. In the spring of 1865, while near Buxa, Bhutan, on the southern slopes of the Himalayas, "the report of a heavy gun was heard in the direction of the mountains, clear and distinct, yet a long way off, followed closely and at irregular intervals by two other discharges. . . . These reports were louder and more distinctly like artillery fire than any I afterwards heard in the hills further to the east. These last had the nature of a very, very distant boom, coming from no well-defined direction."[1] He also speaks of having several times heard noises, like the distant report of heavy guns, in the North Cachar Hills.

Although an Indian village is the eponym of the "Barisal Guns", India has no monopoly of them. Such noises have been heard in the British Isles—on Dartmoor, at several places in Scotland, and, quite frequently, on the shores of Lough Neagh. Here are some notes on the last-named by the Rev. W. S. Smith of Antrim.[2]

"For many years after my settlement here as minister from England, I heard at intervals, when near the lake, cannon-like sounds. . . . In time I came to understand that it was not from the opposite shores, but from the lake itself that the sounds proceeded. After questioning many of the local residents, I extended my inquiries to the fishermen, but they could assign no cause. A strange thing about the matter is that the people generally know nothing of the phenomenon, and that it is shrouded in mystery. . . . I have heard the sounds probably twenty times during the present year,[3] the last being on a Sunday afternoon a month since, when I heard two explosions; but with two exceptions they have all seemed to come from many miles away, from different directions at different times.

[1] *Nature*, 16.i.1896. [2] ibid., 2.i.1896. [3] 1895.

They have come apparently from Toome Bay, from the middle of the lake, and from Langford Lodge Point, about nine miles distant. . . .

"I have as yet spoken to no one who observed any movement of the waters when explosions took place, nor have I spoken to any one who was close to the spot at the time, rather every one seems to have heard them only in the distance, which is strange, as fishermen are on the lake during many months in the year, at all hours of the day and night."

Similar "guns" are often heard off the Belgian coast, where they are locally known as "mist poeffers" (lit. "fog-hiccups"),[1] while they have been reported from many parts of Australia. The earliest account of these which I can trace is that given by Sturt, when describing his great journey of 1828–9, in which he discovered the Darling and Murray Rivers. Encamped near the Darling, in February 1829, he notes in his journal:

"About 3 p.m. on the 7th, Mr. Hume and I were occupied tracing the chart upon the ground. The day had been remarkably fine, not a cloud was there in the heavens, nor a breath of air to be felt. On a sudden we heard what seemed to be the report of a gun fired at the distance of between five and six miles. It was not the hollow sound of an earthly explosion, or the sharp cracking noise of falling timber, but in every way resembled a discharge of a heavy piece of ordnance. On this all were agreed, but no one was certain whence the sound proceeded.

"Both Mr. Hume and myself had been too attentive to our occupation to form a satisfactory opinion; but we both thought it came from the N.W. I sent one of the men immediately up a tree, but he could observe

[1] See a series of articles, covering the whole range of the reported phenomena, by E. Van den Broek, of the Natural History Museum at Brussels, in *Ciel et Terre*, 1895–6.

nothing unusual. The country around him seemed to be equally flat on all sides, and to be thickly wooded: whatever occasioned the report, it made a strong impression on all of us; and to this day, the singularity of such a sound, in such a situation, is a matter of mystery to me."[1]

Mr. H. L. Richardson, of Hillsprings, Carnarvon, W. Australia, reported hearing three explosions high up in the air, followed by a rushing noise like escaping steam (which lasted for several seconds), on June 26, 1908.[2]

It is a far cry from W. Australia to the Rockies; yet here, too, we find the Barisal Guns.

On July 4, 1808, the Lewis and Clark expedition was encamped at Great Falls, Montana, about eighty miles eastward to the main range of the Rockies. The explorers record in their journal:

"Since our arrival at the Falls we have repeatedly heard a strange noise coming from the mountains in a direction a little to the north of west. It is heard at different periods of the day and night, sometimes when the air is perfectly still and without a cloud, and consists of one stroke only, or five or six discharges in quick succession. It is loud, and resembles precisely the sound of a six-pound piece of ordnance at the distance of three miles."

A party equipped by J. J. Astor, the American fur magnate, skirting the Black Hills of Wyoming and Dakota, noted in 1810: "In the most calm and serene weather, and at all times of the day or night, successive reports are now and then heard among these mountains,

[1] *Two Expeditions into the Interior of Southern Australia*, 2nd edit. 1834, vol. i. p. 98.

[2] *Nature*, 27.viii.1908. See also a letter in the same paper, 4.vi. 1908, from Mr. J. Burton Cleland, describing a "dull roar, lasting several seconds", heard by him on August 9, 1907, when encamped on the Strelley River.

resembling the discharge of several pieces of artillery. Similar reports were heard by Messrs. Lewis and Clark in the Rocky Mountains." It may be added that in 1854 a Mr. Doty, when near the point of the Rockies from which the sounds heard by Lewis and Clark seemed to come, heard similar noises, and was certain that they emanated from the mountains. On the other hand, later visitors to the locality do not seem to have heard them.[1]

The Barisal Guns have also been frequently heard in Haiti,[2] where the sound is known as the "gouffre". They have also, though rarely, been known to occur at sea; witness the following entry in the meteorological log of the S.S. *Resolute*, Captain W. Deuchars, for July 30, 1883, 8 p.m. "Six reports like those of guns heard to the westward, supposed to be caused by electricity, as no ships are thought to be in the vicinity." The position given is 71° 09′ N., 12° 28′ W., about sixty miles westward of Jan Mayen Island.[3]

The guns heard at Barisal, then, appear to be only a leading case of a very widespread phenomenon.[4] Many explanations have been suggested—fireworks, actual gunfire, bamboos bursting in jungle-fires, thunderclaps, the collapsing of banks, globular lightning, landslips, submarine eruptions—and a good many more. So far, none of these has been accepted by those who have heard the sounds in the Sunderbunds; or for that matter, in the other localities.

Mr. H. S. Schurr, writing in 1899,[5] pointed out that in

[1] *Nature*, 26.iii.1896.

[2] e.g. in the autumn and winter months of 1912, over the south-western part of the island.

[3] *Nature*, 30.i.1896.

[4] Without extensive investigation, it is impossible to affirm that all the instances quoted are exactly on all fours, and it is not suggested that they are necessarily all due to the same cause: but they have at least a strong family resemblance—and, for all of them, no simple explanation seems adequate.

[5] *Nature*, 7.xii.1899.

his experience the "Barisal Guns" always occur in triplets, with a slight echo-like sound immediately after each report, and a subsequent interval of from three to ten seconds before the next, thus: BANG-bang . . . BANG-bang . . . BANG-bang. He rejected, as inadequate, all the explanations offered.

Quite the most fascinating theory as to the origin of the sounds heard in the Ganges delta is that outlined by Lieutenant-Colonel W. P. Drury, of the Royal Marines, in that masterpiece of Marine fiction, *The Peradventures of Private Pagett.*[1] His hero, who is more of a living character than many real Marines (when these are on duty), is supposed to go adrift in the Sunderbunds in a small dinghy. He is being towed by the steam-cutter, and the painter carries away,[2] unnoticed by those ahead. He is left paddling about aimlessly like a lost dog, despairing of rescue and fortifying himself against impending death by repeating the only portion of the Prayer Book he can call to mind—which happens to be, *A Man may not marry his Grandmother.*

Round the bend in one of the innumerable channels he catches sight of a large stranded vessel. She proves to be one of H.M. ships, wrecked many years before, but still tenanted by a few greybeards commanded by a senile midshipman, the only surviving officer. All suffer from two fixed ideas—that they must never abandon the ship, and that they must fire guns at intervals to scare away wild beasts. He is shown two small signalling guns, and realizes that there are the famous "Guns of Gungapore"—in other words, the Barisal guns. Parting company with these Rip van Winkles more in anger than sorrow, he makes his way, after many adventures, back to his ship—where, curiously enough, no one will believe his story.

It is a good yarn—and it is meant, I imagine, to be

[1] London, 1904. See the story entitled "The Signal Guns of Gungapore".

[2] i.e. the tow-rope breaks.

nothing more. Yet part of it, at any rate, might have been founded on fact. When Wallis, on his way round the world, touched at Batavia with the *Dolphin* in December 1767, he found there another King's ship, H.M.S. *Falmouth*, "lying on the mud in a rotten condition". She had been there nearly ten years. She was worn out, and so were her men. Wallis states that the ship was in so decayed a state that she could hardly be expected to survive the next monsoon—only the mud kept her from sinking at her anchors; while her ship's company consisted of no more than a few men, old and broken. There were no executive officers left, and of the remainder the gunner was dead, the boatswain had gone mad, and the carpenter was dying. The survivors entreated Wallis to discharge them from the hulk which they had tended so long, and to let them embark with him for home; offering to forfeit the ten years' pay due to them, "and go home sweepers, rather than continue the miseries of their present situation". He refused. They had Government stores in their charge, and they must await orders from England as to the disposal of these before they could quit Batavia. Poor fellows, they had never had an order of any kind from England since their arrival ten years earlier. Nothing definite is known of their fate; but Carteret, who was at Batavia in August 1768, incidentally refers to the *Falmouth* as having been condemned.

Despite the assertions of the veracious Pagett, it does not seem likely that the Barisal Guns are due to actual gunfire, or to human agency of any kind. They are probably a natural phenomenon, but whether this should be located in the air, the land, or the sea remains at present an open question. It might very suitably form the subject of an investigation to be conducted by that proposed triple combination of Forces—the Ministry of Defence.

VII

THE CANALS OF MARS

. . . But who shall dwell in those worlds, if they be inhabited? Are we, or they, Lords of the World? . . . And how are all things made for Man?

To these questions, asked long ago by Kepler, there is no ready answer. Nor, indeed, is the need of an answer very apparent; they bear a family likeness to the irritating conundrums of which the Book of Job is full. With most of us, the living of our own lives gives us more than enough to worry about—and if we take an interest in the lives of others (outside our own immediate circle), it is fairly safe to assume that they are persons of some notoriety and, at the very least, that they are, or have been, co-inhabitants of our own little planet.

Some, indeed (chiefly eminent persons of advancing years, duly impressed with a proper sense of their own importance), have stoutly denied the possibility of there being any form of life in any other world than this. Thus in the 'fifties of last century William Whewell, Master of Trinity, published a work entitled *Of the Plurality of Worlds*,[1] for which a better title would have been "*The Creator's Power Limited to this Earth, and the Reasons. By One on the Steps of the Throne*". As has been said, it seemed planned to demonstrate that

. . . throughout all Infinity
There is no one so great as the Master of Trinity.

It stung Sir David Brewster into the composition of an almost equally bizarre reply,[2] *More Worlds than One, The Creed of the Philosopher and the Hope of the Christian*.

[1] *Of the Plurality of Worlds: An Essay*. (Anonymous.) 1853.
[2] Published in 1854.

The antagonists were well matched; it is difficult, even after reading their works with close and painful attention, to discover which should be awarded the palm of ignorance in matters astronomical. Whewell was the more dogmatic; on the other hand, Brewster laboured under the handicap of possessing a firm belief in the literal inspiration of Holy Writ. Peace to their ashes!

In much more recent times the late Dr. Alfred Russel Wallace published a work[1] in which he attempted to show that "the earth is the only inhabited planet, not only in the Solar System, but in the whole stellar universe". It is fair to add that he was eighty when he wrote it. His preface breathes a spirit of serene and inextinguishable self-satisfaction:

> "Having long been acquainted with most of the works dealing with the question of the supposed *Plurality of Worlds*, I was quite aware of the very superficial treatment the subject had received, even in the hands of the most able writers, and this made me the more willing to set forth the whole of the available evidence—astronomical, physical, and biological—in such a way as to show both what was proved and what suggested by it.
>
> "The present work is the result, and I venture to think that those who will read it carefully will admit that it is a book that was worth writing. It is founded almost entirely on the marvellous body of facts and conclusions of the New Astronomy together with those reached by modern physicists, chemists, and biologists. Its novelty consists in combining the various results of these different branches of science into a connected whole, so as to show their bearing upon a single problem—a problem which is of very great interest to ourselves. "

[2] *Man's Place in the Universe*, Alfred R. Wallace, LL.D., D.C.L., F.R.S., etc.; London, 1903.

Actually, the problem raised by Wallace's unfortunate book was this. When an eminent scientist, in his dotage, rushes into print upon a complicated subject which he is no better qualified to discuss than thousands of other people, and endeavours to prove a universal negative by a jumbled mass of theories and opinions selected (according to his preconceived notions) from various conflicting authorities, what inference are we entitled to draw as to the probable value of his work upon his own special subjects?

Moreover, the question of whether any form of life as we know it exists in other worlds is not merely complicated—it is one which cannot be solved by any amount of *a priori* reasoning. The conclusions reached in this way are conditioned entirely by the assumptions which one chooses to make.

If one adopts the attitude that life can only exist under conditions closely analogous to those which obtain on the earth, then it is not difficult to make out a strongly reasoned case for supposing that such conditions are not exactly paralleled anywhere else in the Solar System—or, for that matter, outside it. Such is the method of Whewell, Wallace, and others.

On the other hand, if we can bring ourselves to believe that life is still possible in conditions—such, for example, as a total absence of air, or of water; or temperatures ranging from (say) red-heat to something approaching the absolute zero—which would be immediately fatal to anything which we should regard here as "living", then there is no difficulty in concluding that

> . . . there's not the smallest orb which thou beholdest

which is not densely populated. Such was the method of Brewster, and by its aid he concluded that not only the stars and planets, but also the airless moon, and even the sun, were to be regarded as the abode of life. It is only fair to add that a very great astronomer indeed, Sir

William Herschel, held the same view with regard to the sun.

On such a subject, then, theory is an entirely untrustworthy guide. But direct observation is scarcely better off. The largest and most powerful telescope yet constructed, the Mount Wilson 100-inch reflector,[1] working under the most favourable atmospheric conditions, would scarcely enable an observer actually to see living beings (of any ordinary dimensions) in a body so near to us as our own moon. Such feats are reserved for instruments such as the telescope (apparently of the Rosse type) erected by the Gun Club of Philadelphia on the summit of Long's Peak, Rocky Mountains:[2] or that alleged to have been set up by Sir John Herschel at the Cape in 1836,[3] whose powers were so miraculously assisted, in flat defiance of all principles of optics, by a "transfusion of light through the focal object of vision", and whose reported disclosures of lunar sheep, bat-winged savages of amorous inclination, and tropical vegetation were, for some time, greedily swallowed by many people who ought to have known better.

If, then, analogy gives no sound basis and direct observation fails, must we conclude that there is no way by which we can come to any conclusion as to whether life exists in other worlds? By no means. We may, if we wish, inquire of "the spirits". Here are some "revelations" upon the subject, supposed to have been

[1] A 200-inch telescope of this type is at present (1937) under construction. For planetary observations, however, a large reflecting telescope is not greatly superior to a smaller achromatic, definition in such cases being more important than light-gathering capacity.

[2] According to Jules Verne (*Hail, friend of my youth!*) in his *From the Earth to the Moon.*

[3] The reference is to the once-famous *Lunar Hoax,* published, as serious truth, by R. A. Locke in 1836. It was cleverly put together and, up to a point, convincingly written. The book is very scarce, but a fairly full discussion of it may be found in any complete edition of Poe's works, as an appendix to his *Journey of Hans Pfaall.*

communicated by Sir Walter Scott[1] (of course, he would know) at a séance held in London on June 27, 1895. The medium was the celebrated Mrs. Piper, who professed to call up Scott at the request of Professor W. R. Newbold. Sir Water is describing the sun.

> "Well, now we move on towards this fire, now reach its borders, and notwithstanding the extreme heat we pass through it, and we find ourselves upon a solid bed of hot clay or mud. This is caused by gravity. Understand where we are; we have now reached the limit;[2] we find it very warm and deserted, like a deserted island. . . . Now we see what we term monkeys, dreadful-looking creatures, black, extremely black, very wild. We find they live in caves which are made in the sand or mud, clay, etc. Now, sir, for that I will be obliged to discontinue our journey until some future time."

At a sitting next day the medium, who may have tried to learn a little astronomy in the interval, had another shot, and attempted to explain that the scene described had been on the earth, not the sun. However, worse was to come.

"PROF. NEWBOLD. What are the sun spots?

"SCOTT. This is the shadow of the earth, sir.

"PROF. NEWBOLD. You are thinking of eclipses. I understand this, but I mean the black spots sometimes seen in the sun?

"SCOTT. Oh, I beg your pardon, sir; I did not understand your question-thoughts.

"PROF. NEWBOLD. I beg your pardon.

[1] One wonders why they did not call up Brewster; or, for that matter, Newton, or Herschel, or somebody who might be expected to have better qualifications. In the spirit world, apparently, celebrities, although obviously suffering from softening of the brain, are always ready on tap.

[2] So one would suppose: but the credulity of "spiritualists", unfortunately, is infinite.

"SCOTT. No, sir; I understand now: the spots on the sun are . . . yes, sir . . . are the so-called satellites which surround it; this produces a dark mass of spots."

On being asked about the climate of Mars, he replied, "Very fair: it is in the torrid zone"!

But, apart from these imbecilities, there is certainly one way in which we might be able to obtain evidence pointing to the conclusion that another heavenly body was inhabited. Our present instrumental resources are sufficient to let us scrutinize the nearer planets fairly closely; and if we detected on them any structures of an unmistakably artificial character, it would be a fair proof that intelligent beings existed there. By "structures", I do not mean ordinary houses. But if, say, H.M.S. *Hood* were suddenly transported to the surface of the moon, she would probably be detected by the Mount Wilson Observatory, to the great surprise of its Director and the extreme consternation of Their Lordships the Lords Commissioners of the Admiralty.[1] And, if the Panama Canal were transferred there, and filled with water (there is none in the moon) the same telescope would probably show it in its whole length, and would certainly pick up the Culebra Cut and the Gatun Locks. Indeed, it was suggested many years ago by J. von Littrow,) a German mathematician, that we should attempt to communicate with the planets by outlining with lights, in some such place as the Sahara or the Russian steppes, geometrical figures (such as Euclid I. 5 or I. 47)[2] of colossal size. The proposal, however, met

[1] We must, however, remember the saying: "There is nothing the Navy cannot do." I do not think that the Admiralty have ever contemplated operations in the Moon; but it is a fact that in 1812 they proposed to send a frigate up the Falls of Niagara for the purpose of reinforcing our squadron on the Great Lakes.

[2] The famous "Asses' Bridge", and the equally famous theorem that the square on the hypotenuse equals the sum of the squares on the other two sides. I am not sure which of the eighty-odd known proofs of this proposition it was intended to employ.

with no response. Its utility was highly questionable, while its probable cost was sufficient to appal the most stout-hearted member of the I.L.P. (which, however, was not then in existence).

Now it so happens that on one of the nearer planets, and the one whose surface can be most easily observed,[1] many observers believe that they have detected structures of an artificial character—the celebrated Martian "canals". If we accept their observations and deductions, we must conclude that Mars is covered by a fine network of artificial canals, the work of creatures of a very high order of intelligence and possessed of engineering abilities and resources much in advance of our own. These resources they have devoted to what must be the greatest engineering work ever accomplished—an irrigation system designed to conserve and distribute their ever-dwindling water supply; a standing monument of a magnificent resistance offered by intelligence to the cruel fate which must, ultimately, overtake life in our own planet, and which has pressed more insistently upon the Martians because, so far as we can judge, their planet is both smaller and older than ours.

It is a fascinating theory, which admits of being worked out in great detail. For example, Mr. C. E. Housden, in a work published in 1914, has[2] gravely discussed the hydraulic problems of the hypothetical Martian canal system. His monograph would, no doubt, be of considerable assistance to an engineer who contemplated applying for a post in this undertaking. But it is open to one grave objection, which is of a rather formidable character. No clear and unquestionable proof has

[1] The only nearer planet at any time (except one or two asteroids) is Venus, with a cloudy atmosphere, and never well-placed for detailed observation. It is fairly certain that the giant planets—Jupiter, Saturn, Uranus, and Neptune—are worlds in the making, which are gradually cooling and consolidating. Mercury is subject to the same observational obstacles as Venus.

[2] *The Riddle of Mars the Planet*, C. E. Housden, London, 1914.

yet been given that the Martian canals—understanding by that word artificial structures of any kind—really exist.

It may be as well to recall a few facts about Mars itself. It is much smaller than our earth, having a diameter of a little over 4,000 miles.[1] Like ourselves, it revolves around the sun, but farther off—some 50,000,000 miles farther—and takes nearly two of our years to complete its circuit. About every two years and fifty days, on the average, Mars and the earth are in opposition—that is to say, in line, or practically so, viewed from the sun: but Mars' orbit is so eccentric[2] that the actual dates of these oppositions—which are the most favourable opportunities for examining the surface of Mars—and the distance between the planets on such occasions, vary considerably. In the most favourable circumstances, Mars' distance when in opposition is about 35,000,000 miles.

Now, as astronomical distances go, 35,000,000 miles is not very much: it is, for example, less than half the distance from the earth to the sun, while in comparison with the distance of some of the nearest stars it is, as Sir Boyle Roche once said (or did not say), "a mere flea-bite in the ocean". But, at the same time, it militates very seriously against our examining the surface of Mars in detail. There are comparatively few astronomical telescopes now in existence which give as good a view of Mars, even when in opposition, as can be obtained of the full moon with the naked eye:[3] and quite an ordinary single-draw hand telescope will give a better view of the latter than can be got of Mars with any telescope yet constructed.

[1] About 4,250 miles.

[2] It was this eccentricity—greater than that of any other planet except Mercury—which induced Kepler to discard the old theory that the planets revolved in circles: a step which led him to discover his famous three "Laws".

[3] In other words, the apparent disc of the planet looks considerably smaller than a threepenny-bit held at arm's length.

The earliest telescopic observations of Mars (made with telescopes of small power) revealed, so far as can be judged by the drawings which have survived, very little real detail;[1] and Sir William Herschel's drawings, made with a large reflector not very well adapted for planetary work, have been described as caricatures. Beer and Mädler (1830–7) produced the first reasonably accurate chart of the planet's principal features.

By the middle of the nineteenth century, it was generally agreed that Mars exhibited snow-caps at its poles which almost entirely disappeared, alternately, during the Martian summer of their respective hemispheres; that it had seasons resembling our own; and that on its disc could be traced large areas of fluctuating outlines, some being of a reddish-yellow colour, while others were darker, appearing of a neutral tint to some observers and a greenish to others. It was assumed that the former portions were land and the latter (which seemed to be united in various places by narrow straits) seas. Such, in general, was the state of areography (the mapping of Mars) at the time of the memorable opposition of 1877.

It was memorable for two reasons. Mars had for long been regarded as possessing no satellites,[2] except in the imagination of Swift[3] and Voltaire[4] (both of whom had stated, long ago, that it had two). But on August 11–17, 1877, Asaph Hall, working with the Washington 26-inch refractor, succeeded in detecting two tiny attendants of Mars, which he very aptly named Deimos and Phobos.[5] They are absurdly small, Phobos being about a dozen miles in diameter, and Deimos about half as

[1] It should be gratefully remembered, however, that drawings made by Hooke (1666) and Huygens (1672) have proved most serviceable in determining Mars' rotation-period, which is practically the same as that of the earth (actually, 24h. 37m. 22·67 secs.).

[2] Tennyson, for example, sang of ". . . the snowy poles of moonless Mars." I believe he altered this, later, to ". . . the snowy poles and moons of Mars."

[3] In *Gulliver's Travels*.

[4] In *Micromegas*.

[5] Fright and Panic—the Homeric names of Mars' chariot-horses.

much; but, if small, they move quickly. Deimos makes his circuit in about thirty hours, and (the Martian day being much the same length as ours) stays in the sky for days at a time; while Phobos presents the unique spectacle of a satellite revolving round its primary more than three times as fast as the latter rotates, so that it rises in the west and sets in the east.[1]

It was during the same opposition that G. V. Schiaparelli,[2] of Milan, discovered a number of fine streaks on the Martian disc which had not been charted by any previous areographer. These he termed "*canali*"[3]—"channels"; a perfectly suitable term, not implying any artificial origin. That other observers, with better instruments than Schiaparelli's 8½-inch refractor, should have failed to notice these "canals", as they rather unfortunately came to be called, seemed a little surprising, but it is now known that, whatever the nature of the phenomenon, most of the canals are best seen at oppositions when Mars is not very favourably placed for observers in the higher northern latitudes.

A few of Schiaparelli's canals were confirmed by old drawings made by such observers as Dawes and Green, on which indications of them could be traced; but most of them had to wait for even a limited acceptance until 1886, when they were independently[4] observed by Perrotin and Thollon with the Nice 15-inch telescope. In the interim, it was generally supposed that Schiaparelli had been the victim of an optical illusion or of an unusually vivid imagination; and support seemed to be lent

[1] During Scott's second Antarctic expedition, one of his parties had the curious experience of seeing the sun set in the morning and rise the same afternoon. They were a little west of 180°, and kept G.M.T.

[2] Born 1835; Director of the Milan Observatory 1862–1900; died 1910. During the last years of his life he was totally blind.

[3] The term had been used, in like manner, by Secchi in 1859.

[4] So we must believe: but the canal-chart produced by the Nice observers is almost indistinguishable from Schiaparelli's own (which was available to them) and differs widely from more recent observations.

to this view when he announced that at the opposition of December 1881, he had observed that as many as thirty of the canals which he had previously observed as single streaks had now proved to be *double*, the two streaks running parallel. In certain cases a canal was seen as single one night, and as double the following night; while the distance apart of the pairs amounted, in extreme cases, to as much as 500 miles.

However inexplicable, this duplication was also confirmed by Perrotin and Thollon, and it gradually came to be accepted that the canals were not figments of Schiaparelli's imagination, and that the failure of observers in general to see them was due simply to the fact that they lacked one or more of three essential requisites which the Italian astronomer possessed—a good telescope, great skill in observation, and favourable atmospheric conditions. Opinion swung round, and before very long observers were competing as to the number, size, and extent of the single and double canals which they could find, and make room for, on their Martian charts (see Fig. 14).

Easily the most prominent of these observers was the late Dr. Percival Lowell. Before he erected his famous observatory at Flagstaff (Arizona), Lowell visited France, Algeria, and various American sites, always looking for clear conditions of atmosphere. As he himself remarked:

> "A steady atmosphere is essential to the study of planetary detail, size of instrument being a very secondary matter. A large instrument in poor air will not begin to show what a smaller one in good air will; when this is recognized, as it eventually will be, it will become the fashion to put up observatories where they may see rather than be seen."

He began his life-work at Flagstaff with an 18-inch refractor, to which a 24-inch refractor and a 40-inch reflector were afterwards added. Certainly, as regards

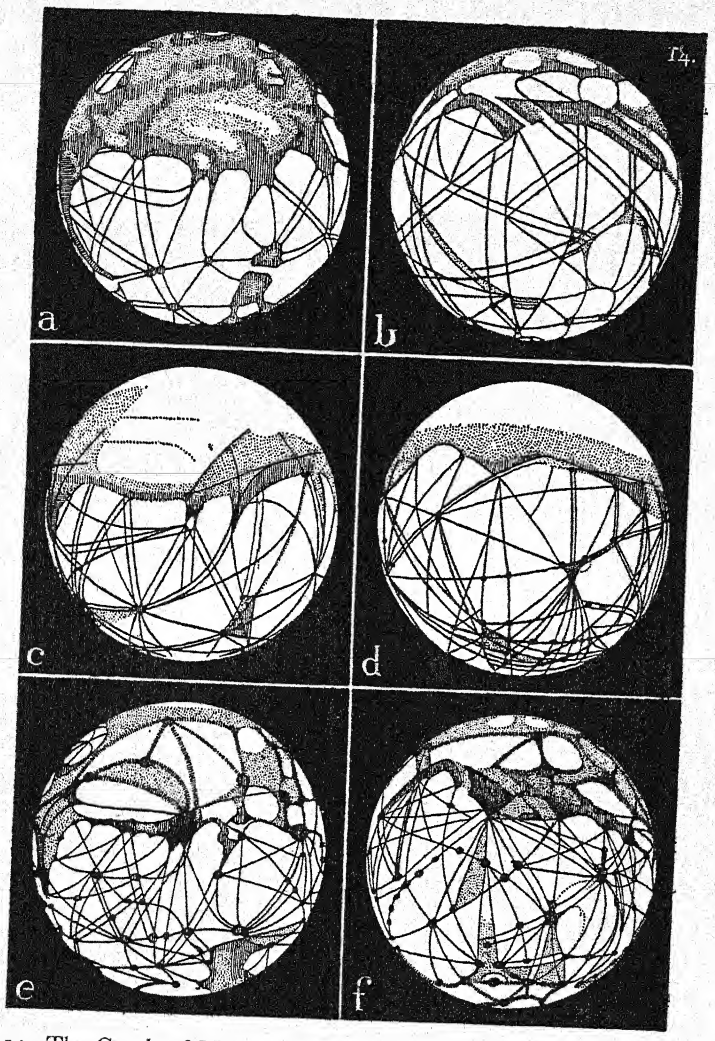
instrumental equipment and favourable conditions for observation, his observatory had, and has, few rivals.

Lowell's results, which he embodied in a large number of scientific papers issued as periodical *Lowell Observatory Bulletins*, and also in three popular works,[1] were of a very surprising kind, and it is not surprising that few were found who could accept them in their entirety. As depicted by him, the disc of Mars was covered with a perfect spider's web of canals, some 700 in all, of an almost rigidly geometrical character. Many of the canals were duplicated (the components, in general, being strictly parallel), and at many places where one canal intersected another appeared a large dark spot (termed an "oasis") usually of a strictly circular shape. Upon his charting of Mars Lowell erected and defended an elaborate argument, almost as ingenious and complicated as his canal system itself, demonstrating that the canals were unquestionably planned to serve as an irrigation system, and that they could be nothing else but the work of living and intelligent beings.

Granting the premises, his conclusion seems eminently reasonable. Many of his opponents wasted a lot of ink and paper in laborious attempts to show, as Whewell might have done, that there was no water on Mars; or, at least, not enough to fill the canals; or that the mean temperature on Mars was such that the water would always be frozen, and so on. To my mind, such reasonings, even if sound in themselves, are beside the point. If the canals, as drawn, exist, they are almost certainly artificial. If they are artificial, then, whatever their purpose, they prove that there is intelligent life in Mars.

Actually, many of the *a priori* arguments have now been abandoned. Dr. W. H. Pickering, of Jamaica, who has devoted many years to the study of Mars, and who has organized a systematic and continuous examination

[1] *Mars*, 1895; *Mars and its Canals*, 1906; *Mars as the Abode of Life*, 1909.



14. The Canals of Mars, as mapped (in identical hemispheres) by three of their most prominent exponents.
a, *b*, after Schiaparelli. *c*, *d*, after Lowell. *e*, *f*, after Maggini.

of its details by various observers in different countries, has summed up the position thus:[1]

> "I believe it is now time for planetary astronomers to change their views with regard to surface conditions on that planet, and adopt the position that not merely its temperature, but also its atmospheric pressure, closely resembles that found on the earth.[2] Heretofore we have doubtless all felt that it was possible that it supported animal as well as vegetable life, and even that intelligent life, if not proved, was not impossible. Now, however, we may perhaps say that with similar conditions to those found on the earth, and vegetable life assured, animal life is almost certain. Furthermore, if it, and if intelligent life exist there, as the straight and narrow canals seem to imply, then the evidence now adduced indicates that it need not be so very unlike ourselves as we have heretofore been led to surmise."

But Lowell's opponents—and a man of such outstanding personality and novel views was bound to make opponents—were (and, for that matter are) on much stronger ground in questioning the objective reality of the canals *as drawn by him.*

We see with the brain, not the eye. The eye is only an instrument, and not a very perfect instrument at that; in fact, Helmholtz once said that if his instrument-maker were to send him an instrument as badly designed as the human eye he would send it back, and decline to pay the carriage. The function of the brain is to put the right interpretation on the messages which it receives from the eye: and interpreters, however competent, are never infallible. It is often very difficult to determine exactly how much we see, and how much of what we think we

[1] *Report on Mars*, No. 30, W. H. Pickering (Mandeville, Jamaica, December 20, 1924).

[2] This view has since found further support in observations made with thermo-couple apparatus in 1926 by various observers.

see is supplied by our imagination; and that is the crucial difficulty in deciding as to the reality of the Martian canals.

The expert can see more with half an eye than the novice with two. Agassiz, the American naturalist, used to give a student some quite simple fossil to study, and ask him to say when he thought he had seen all he could in it. This usually happened in about ten minutes, after which Agassiz would show him something quite obvious and important which he had overlooked. The process would be repeated until (in about two days) the student had learned to appreciate the difference between seeing and observing.

Similarly, the campanologist can at once detect all sorts of overtones and partial tones in the note of a bell; refinements which the ordinary man fails to appreciate simply because he does not know what he should listen for. And the same, *mutatis mutandis*, holds good of all our senses—the right use of them no more "comes by nature" than, as Dogberry imagined, do reading and writing.

On the other hand it is possible to be over-subtle, and to get into a state of "expectant attention" in which the brain outruns the senses and reads into their messages more than these contain. And, in consequence, while it is true that a novice at areography might fail to see canals on the Martian disc which, to the eye of a trained observer, were staring him in the face, it is no less true that the latter might persuade himself that he saw canals where he actually saw disconnected marks which, he was convinced, formed a continuous line.

There is a well-defined limit, for any person, as to what he can see clearly enough to appreciate its shape correctly. Below that limit one tends to see objects, whatever their real shape, in one of two forms—lines or spots. And it must be remembered that in straining one's vision—as, even in the best conditions, one must—to make out the Martian canals, one is putting a considerable

tax upon both the eye which receives the impression and on the brain which interprets it. It is not surprising, therefore, if the result is, to some extent, an illusion.

Perhaps I may be allowed, in this connection, to mention an experience of my own. The drawing of Fig. 14 necessitated a good deal of work under conditions somewhat resembling the actual mapping of Mars through the telescope. The original maps which I used had to be re-drawn on the orthographic projection,[1] which entailed tracing the path of each canal through a set of some four hundred squares occupying an area of about four square inches. Each tracing was then reduced and inked-in, viewing it through the paper by transmitted light, in a darkened room. The work occupied about a fortnight, and at the end of this period I found that I was beginning to "see canals" in all sorts of places—in the foliage of trees, in the shadows dappling a garden path and, almost curious of all, on the smooth and well-lit surface of a billiard table. After a few days' rest, this illusion—undoubtedly a product of eye-strain—disappeared.

The late E. W. Maunder, in his *Are the Planets Inhabited?*[2] has put the case for the illusory nature of the canals very clearly:

> "It is sufficient, then, for us to suppose that the surface of Mars is dotted over with minute irregular markings, with a tendency to aggregate in certain directions, such as would naturally arise in the process of the cooling of a planet when the outer crust was contracting above an unyielding nucleus. If these markings are fairly near each other it is not necessary, in order to provide the effect of 'canals', that they should be individually large enough to be seen. They may be of any conceivable shape, provided that they are separately below the limit of defined vision, and

[1] Schiaparelli's map was on the stereographic projection; Lowell's and Maggini's, on Mercator's.

[2] London, 1913. Pp. 102, 103.

are sufficiently sparsely scattered. In this case the eye inevitably sums up the details (which it recognizes but cannot resolve) into lines essentially 'canal-like' in character. Wherever there is a small aggregation of these minute markings, an impression will be given of a circular spot, or, to use Prof. Lowell's nomenclature, an 'oasis'. . . .

"The above remarks apply to observation with the unaided eye, but the same principle applies yet more strongly to telescopic vision."

Maunder, with Mr. J. E. Evans,[1] carried out a rather striking experiment in support of his views. A number of boys at Greenwich Hospital School were set to copy what they could see of a design, similar to a map of Mars, pinned to a blackboard. The design was tinted to correspond with the general outlines of the Martian "seas" and "continents", but had no definite lines of any kind on it. The drawings produced by the class varied in a very peculiar manner. The boys sitting nearest the board reproduced the design practically in facsimile. Those at the back of the room did much the same, but of course less accurately. In neither case was anything extraneous introduced. But some of the boys at intermediate distances also thought that they saw (and accordingly drew) lines which resembled the Martian "canals". It should be added that none of them had any knowledge of the question at issue, nor was anything hinted as to what they ought to see. But it must be admitted that, while suggestive in its result, the conditions of the experiment were so different from actual observation that, as Lowell pointed out, the fact that some of the boys were mistaken in what they thought they saw did not prove that he had been.[2]

[1] E. W. Maunder and J. E. Evans, *Monthly Notices, R.A.S.*, lxiii, 488, June 1903.

[2] He afterwards claimed to have proved, by a very similar experiment, that his canals were undoubtedly no illusion!

Still, it is difficult to escape the conclusion that Lowell's drawings, and those of his assistants and disciples, owe their peculiar and distinctive look to a convention which he adopted (consciously or otherwise); that, in short, he founded a "Lowell School of Areography". I have not seen any of the drawings produced at Flagstaff since Lowell's death in 1916, but much of his influence is undoubtedly to be traced in the Martian drawings of Dr. Maggini, Director of the Royal Observatory, Catania (see Fig. 14).[1] The "spiritual home" of a Maggini drawing is as distinctly Flagstaff as that of a Maggini violin is Brescia. In this connection a remark of Pickering's, as to the Maggini drawings published by him, is very interesting:

> "He [Dr. Maggini] says, for instance, that he could make drawings of Mars closely resembling those of the other observers if he chose to do so. That is to say, their drawings resemble the planet as he sees it. He prefers, however, to add to the general outlines and canals that he readily sees, certain other features which are extremely faint and difficult, so difficult in fact that in order to show them at all on paper their intensity must be greatly exaggerated—exaggerated out of all proportion to the other detail. This of course destroys the resemblance of the drawings to the planet. This statement at once explains a good deal, and I believe really solves the question."[2]

It certainly explains a good deal, but it must be remembered that Lowell never made any admission of the kind. Yet the history of areography has many instances of structures—at first drawn in all good faith, as Schiaparelli and Lowell drew the canals and oases, as simple, regular, geometrical lines and circles—gradually becoming

[1] It is fair to say, however, that Dr. Maggini has distinctly stated that he does not accept "the Lowellian theories of Mars".

[2] *Report on Mars*, No. 25, 1922.

resolved, by patient examination, into finer and more complex detail. With the gradual advance of telescopic power, it seems not unlikely that even the larger canals, whose existence is undoubted, may be found to present a very different appearance, and that the smaller may be resolved, as Maunder suggested, into irregular markings. E. M. Antoniadi, whose work on Mars with the great 32·7-inch refractor of the Meudon Observatory near Paris is well known, has gone so far as to say:

> "The conclusion is that if, by the canals on Mars, we mean straight lines, then they certainly have no existence; but if, by canals, we mean irregular lines of complex structure which have been produced by natural causes, then their existence is undoubted."

And it may be recalled that while Lowell claimed, in 1897,[1] to have observed similar canal-like markings on Venus, "perfectly distinct" markings, whose contours "had the look of a steel engraving", he spoke of the same markings in 1906[2] as "hazy, ill-defined and non-uniform", while later he gave them up as an optical illusion—although, later still, he once more asserted that they were objective! Again, Dr. A. E. Douglass, who was Lowell's chief assistant at Flagstaff from 1894 to 1901, stated in an article published in 1907[3] that he considered many of the faint canals mapped at Flagstaff to be illusory. It may be added that Pickering has shown that, by using a very low power, it is quite easy to "see" canals on the surface of the moon.[4]

One might think that if, as shown by Fig. 14, there is so much difference in the drawings made of Mars, under exactly the same aspect, by different observers, and if there is, in consequence, so much variation

[1] *R.A.S. Monthly Notices*, March 1897.
[2] *Mars and its Canals*, pp. 178f.
[3] *Popular Science Monthly*, May 1907.
[4] *Report on Mars*, No. 6.

in the maps of Mars based on those drawings, the obvious course would be to have recourse to photography. This has been done, but unfortunately the results (or, at least, such results as are generally accessible) are almost as difficult to interpret as the Martian disc which they depict.

The first photographs claiming to show the canals were, appropriately enough, taken at Flagstaff. After various unsuccessful attempts, Mr. C. O. Lampland, using a colour screen and with the aperture of the telescope reduced by a diaphragm, succeeded in obtaining a number of photographs, of which six were published in the *Lowell Observatory Bulletin*, No. 21.

To judge by Lowell's accompanying remarks, headed

"THE CANALS OF MARS—PHOTOGRAPHED"

the existence of the canals had been triumphantly vindicated. He says:

> ". . . The negatives thoroughly confirm the eye in showing not only the existence of the canals but the fact that they are continuous lines and not a synthesis of other markings. Beyond a certain magnification, if a magnifier be used to examine them, the grain of the plate will show. This must not be taken for discontinuity in the image.
>
> "Two points are worthy of notice:
>
> "1. The corroboration of the fact of the canals by the photographs.
>
> "2. The corroboration of the methods found most efficient to their detection visually.
>
> "PERCIVAL LOWELL"

But at first sight it is difficult, on examining the photographs, to take them seriously.

To begin with, they are quite minute. The disc of Mars is just a quarter of an inch across. Three such

discs, arranged clover-leaf fashion, could be covered by a threepenny-bit.

Secondly, they are not at all sharp in definition, and four of the six so dark that it is difficult to make anything of them.[1]

Thirdly, while on the other two it is certainly possible to detect the main outlines of some of the larger Martian continents—if such they be—it is utterly impossible to detect anything remotely resembling a canal. In view of the scale, no one of ordinary intelligence would dream of looking for such a thing. And yet Lowell claimed, quite seriously, that eight canals and an oasis ought to be recognized with the aid of a map of Mars. What was really needed was a pair of very rose-coloured spectacles.[2]

Larger and better photographs have since been taken at Mount Wilson, and at the Yerkes and Lick Observatories; but on those which I have been able to examine there is no vestige of any canals.[3] One could not expect to see them, for on the largest photograph the Martian disc is less than an inch in diameter; and, of course, they will not stand much enlargement. Lowell's, by the way, were enlarged 1·4 times, so that on the plate the diameter of Mars would have been 0·18 inches only.

It is fair to say, though, that photographs which actually show at least some of the canals have recently been taken at Flagstaff, although I believe that these have not yet been published. I had some correspondence on this point a short time ago with an American friend, and subjoin a few extracts.

[1] It should be noted that they are actual positives, and not reproductions by any mechanical process.

[2] I think I ought to say that I have examined these photographs carefully, both with and without a Coddington lens. I do not need glasses, and I will back myself at any time to write the Lord's Prayer in capitals, without a magnifier, on a disc considerably smaller than a threepenny-bit.

[3] There are certainly one or two streak-like markings, possibly 3–400 miles wide or so—but nothing which in the least resembles the straight, narrow spider-lines depicted by Lowell.

(*J. S. to R. T. G.*, 24.11.1928.)

"I have seen these photographs,[1] and there is no doubt that the canals (when I say 'canals' I really mean 'the markings that we call "canals" ') are there. But the best photographs of them are those made at the Lowell Observatory, in Arizona. I have been there several times, the latest being but a few months ago, and Dr. Slipher and his colleagues have shown me their actual negatives. . . . I think that my experience is especially significant because I firmly believed them to be due to optical illusions until I made my first visit to Flagstaff in 1923, and was not convinced of their objective existence until I saw the negatives.

"So far as Lowell's interpretation of them goes I am still quite sceptical, for I think it quite likely that these markings have some natural origin. . . ."

(*R. T. G. to J. S.*, 19.12.1928)

"Did the negatives you saw show any duplication? I ask this, because to my mind the duplication is the *experimentum crucis*. And, if you could identify any particular canal in different positions on two or more negatives, did it always appear straight, or did it curve, as a natural marking should do, to an amount varying with its distance from the centre of the disc?"

(*J. S. to R. T. G.*, 18.1.1929)

"As regards the 'gemination' of the canals, I must say that I have not seen this on any negatives. I have seen the statement that they have been photographed in this condition, but I am unable to verify this from my own experience. Nor could I say that the photographs of the canals show them curved at the side and straight in the centre as a real marking on the planet should be. However, the canals can only be

[1] The Lick photographs.

photographed well, it appears, when they are in the centre of the disc. The greater thickness of the planet's atmosphere seems to obscure them when they are near the edge, so that I doubt if this test could be applied very conclusively. Of course, the drawings show them curved at the side as they should be, and I, for one, am willing to accept as accurate these drawings by experienced observers."

No doubt, many others will say the same, and quite justifiably so. But, as previously explained, the trouble is that against such drawings can be set a very large number of others whose provenance is equally unassailable—and which, while they depict the planet under precisely the same aspect, show no canals at all. The existing body of evidence from drawings, while it is insufficient to settle the existence of the canals, speaks with no uncertain voice on at least one point. And that is, that such evidence is entirely unreliable. The drawings, pro and con, cancel each other out, and any conclusion based upon them is really determined by the selection of the drawings to which one decides to pin one's faith. It is this regrettable, but not incomprehensible, fact which makes it so vitally necessary to obtain, if possible, clear photographs of at least a good many of the canals.

It is not, as a matter of fact, absolutely certain that, with our present photographic methods, we should be able to say with confidence that if a canal appeared as such on the plate, it was so in reality. It seems possible that, when dealing with faint detail not far from the limit of visibility, detached spots may run together, so to speak, in the lens as they do in the eye; and there is, moreover, the grain of the emulsion to consider. This might, possibly, be avoided by taking our photographs on silver, after the manner of the old daguerreotype; but, unfortunately, a long exposure (such, for example, as is used for photographing faint stars) is out of the question by reason

of Mars' rotation. However, the problem is one which we may legitimately hope to see solved in a comparatively short time; and, whether the famous canals are proved to be real, or partly real and partly illusory, or wholly illusion, there can, I think, be only one opinion as to the skill and devotion shown by the many astronomers who have already done so much towards the solution of one of the most fascinating of the many astronomical enigmas.

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[*f* signifies "foot-note"]